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VOLUME 2, 1912

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FORMATION OF ASSOCIATIONS IN THE MAY-FLY NYMPHS *HEPTAGENIA INTERPUNCTATA* (SAY)

J. E. WODSEDALEK

From the Zoölogical Laboratory of the University of Wisconsin

Two figures

PROBLEM

While working on the life history and general behavior of *H. interpunctata*, I observed, on several occasions, what appeared to be indications of learning. These cases suggested several forms of experiments. Three different ways of testing the learning capacity of the nymphs were devised and carried out, namely, experiments on reactions to objects in their thigmotactic relations, experiments on fear reactions as results of associations with pain, and experiments on the formation of associations in feeding. A considerable amount of work has been done on the learning capacity of higher insects, but the

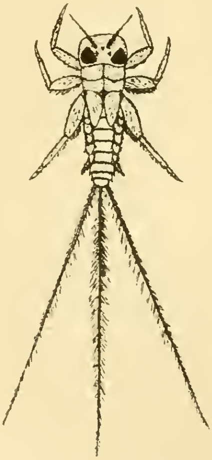

FIGURE 1. May-fly nymph

formation of associations in lower insects has hitherto been the subject of very few experimental investigations.

REACTIONS TO OBJECTS

Different nymphs vary greatly in their response to objects under similar conditions. Some, for instance, will go as far as 11 inches in a direct line toward a stone about one cubic inch in size, while others seem to be unaware of the object even when it is much nearer.

I experimented with 20 specimens in a large glass basin, evenly lighted and evenly colored. The nymphs were placed, one at a time, in a small glass passage, represented by AB in the diagram, within and close to one side of the large basin, and a stone a little more than one cubic inch in size was placed about a foot away from the opening of the glass tunnel.

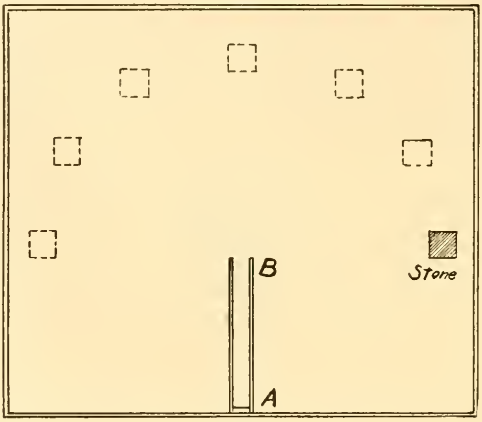

FIGURE 2. Diagram of apparatus used in the experiments on reactions to objects

The little glass tunnel was to prevent random movements. Specimens dropped into water, owing to excitement, are apt to swim about in any direction. A specimen would be carefully placed at the closed end of the passage, which was sufficiently wide to enable it to move freely forward, but which would not permit it to turn around. Slight stimulation from behind induced it to swim forward. In doing so it had ample time to get a view of the enclosure before it reached the open end of the tunnel. If the nymph, after emerging from the tunnel, did not go in a straight line in the direction of the stone, the stone would be brought closer at each trial until a distance was obtained at which the insect would make several successive straight trips to it. The stone would then be placed at the same distance, but at various points in a semi-circle in front of the opening, at different times. Table I shows the variations in the distance at which the first definite trip was made by each one of the different individuals experimented with, and also the percentage of straight trips made by each individual toward the stone, at the recorded distance, in 10 successive trials.

TABLE I

INDIVIDUAL VARIATIONS AMONG H. INTERPUNCTATA IN REACTIONS TO OBJECTS IN AN EVENLY ILLUMINATED ENVIRONMENT

Specimen	Distance in inches	Percentage of straight trips
1	11	70
2	9.5	90
3	9	90
4	8.5	100
5	7	80
6	7	70
7	6.5	100
8	7	80
9	6	90
10	6	80
11	6	80
12	6	70
13	5.5	100
14	5.5	80
15	5	90
16	5	70
17	4	100
18	4	80
19	3.5	100
20	3	70

In another experiment on variations the same apparatus was used, but the source of light was a 16 candle-power incandescent lamp attached to one end of the basin. In order to reach the stone the nymphs were obliged to swim directly toward the light.[1]

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[1] Wodsedalek, J. E. Phototactic reactions and their reversal in the May-fly nymphs *H. interpunctata* (Say). Biological Bulletin, 1911. vol. XXI., pp. 265–272.

Normally the nymph is decidedly negative in its phototactic response. Fifteen specimens were used, one after another, beginning with the stone one inch away from the opening of the small glass tunnel, on the side toward the light, and then increasing the distance until all of the nymphs reacted negatively.

Here again, we have considerable variation, as is shown in table II. For example, all of the 15 specimens went toward the stone, against the light, when it was between one and two inches away from the opening of the tunnel, and when it was between three and four inches away only about half of them went toward it, while, when the stone was placed six inches away, all of the specimens went in the opposite direction.

The uniformity manifested by the nymphs in their negative response to light when no objects are present, and the obvious variations evinced in their reactions to objects in an evenly lighted environment and also in their reactions to objects against the rays of light, seem to suggest that the specimens going the greatest distance toward the objects have learned to do so through experience in their natural environment.

TABLE II

Variations Among Nymphs in their Response to Objects Against the Rays of Light

Distance	Toward stone, against light	Away from the light
1 inch	15 trips	0 trips
1.5 "	15 "	0 "
2 "	14 "	1 "
2.5 "	13 "	2 "
3 "	11 "	4 "
3.5 "	9 "	6 "
4 "	3 "	12 "
4.5 "	1 "	14 "
5 "	1 "	14 "
5.5 "	1 "	14 "
6 "	0 "	15 "
6.5 "	0 "	15 "

In order to ascertain whether the reactions to objects are modified by experience, three of the nymphs having similar records in going toward a stone in an evenly lighted environment were taken and with each 21 daily experiments were performed. At the beginning of each set of trials the stone was placed at the greatest distance at which the nymphs went toward it on the preceding day. Usually, the stone had to be brought somewhat closer than this before the nymph made its first trip toward it. Then the stone was taken farther away after each successful trip, until a distance was reached when the nymph seemed to pay no attention to the object. This necessarily had to occur during each set of trials in order to ascertain the greatest distance at which the nymphs would go to the stone. An over-rating of the distance necessitated the bringing back of the stone to a place where another successful trip was made. The distance made each day on the twelfth successful trip was recorded. The results of this test of the learning ability of three individuals are given in table III. The number of inches indicate the distance each nymph would go toward the stone at the end of 12 successful trials. It will be noticed that two of the specimens increased their distance slowly but steadily, while one varied in its daily behavior and even at the end of three weeks apparently had learned nothing.

TABLE III

Showing the Learning Capacity of Three *H. Interpunctata* Nymphs

Date	Number I	Number II	Number III
1	7 inches	6.5 inches	6.5 inches
2	7 "	7.5 "	8 "
3	7.5 "	7.5 "	8. "
4	8 "	8 "	5.5 "
5	8 "	8 "	7 "
6	7.5 "	8.5 "	8.5 "
7	8 "	8 "	6 "
8	8 "	8.5 "	8 "
9	8.5 "	9 "	7.5 "
10	8.5 "	9 "	5 "
11	8.5 "	9.5 "	8 "
12	9 "	10 "	7.5 "
13	9 "	9.5 "	8 "
14	8.5 "	10 "	8 "
15	9 "	10 "	5 "
16	10 "	11 "	7.5 "
17	9.5 "	10 "	6 "
18	10 "	11 "	8 "
19	10 "	19.5 "	6 "
20	10 "	11 "	7 "

Then 10 specimens that had poor records in going toward a stone against the light were taken and 21 daily experiments were performed with each. The apparatus of the foregoing experiments was used, except that the 16 candle-power incandescent lamp was again introduced at one end of the basin and the specimens, in order to swim toward the stone, had to go directly toward the light. The method of procedure was practically the same as that of the previous investigations. The results were surprising, for most of the specimens gradually inhibited their normal negative phototaxis and proportionately reinforced their positive reaction to the stone. However, the individuals varied considerably. Some showed a slow but steady progress for almost three weeks; some were quite docile for a few days, but soon reached a limit; others learned very little; still others would go toward the stone a fair distance at the end of each set of 21 trials, but had to be started at almost the same distance each day. The four different classes of behavior are represented in table IV.

TABLE IV

Showing Four Different Cases of Learning to Swim Toward Objects Against the Rays of Light

Day	Specimen I		Specimen II		Specimen III		Specimen IV	
	First Trip	Last Trip	First Trip	Last Trip	First Trip	Last Trip	First Trip	Last Trip
1.....	1.5 in.	3 in.	1.5 in.	4 in.	1 in.	2 in.	1 in.	4 in.
2.....	2 "	3 "	2.5 "	5.5 "	1 "	1.7 "	1 "	4.5 "
3.....	1.7 "	3.5 "	2.5 "	6 "	1 "	2.5 "	1 "	5.5 "
4.....	2 "	4 "	3 "	6 "	1.2 "	3 "	1.2 "	6.5 "
5.....	2.5 "	5 "	3.5 "	6.5 "	1.5 "	3 "	2 "	7 "
6.....	2.5 "	4.2 "	3 "	7 "	1.2 "	2.5 "	1.5 "	7 "
7.....	2.7 "	5 "	4 "	7 "	1 "	3 "	1 "	6.5 "
8.....	3 "	5 "	4 "	7.7 "	1 "	3.5 "	1.2 "	7.5 "
9.....	3 "	5.5 "	3.5 "	8 "	1.5 "	2.5 "	1.7 "	8 "
10.....	3 "	5.7 "	4 "	7.2 "	1.5 "	2.7 "	2.5 "	7.7 "
11.....	2.7 "	6 "	3.7 "	8 "	2 "	2.5 "	1.5 "	7 "
12.....	3.7 "	6 "	4 "	8 "	2 "	3 "	1 "	6.5 "
13.....	3.2 "	6 "	4.5 "	8.2 "	2 "	2.7 "	3.7 "	8 "
14.....	3 "	6.2 "	5 "	8 "	1.7 "	3.5 "	2 "	7.2 "
15.....	3.5 "	6.5 "	4.5 "	8 "	1.7 "	3.2 "	1 "	6 "
16.....	3.5 "	6.5 "	4. "	8 "	2 "	3 "	3 "	8.5 "
17.....	4 "	6.5 "	5.2 "	8.5 "	2.5 "	3 "	2.5 "	7.7 "
18.....	3.5 "	7 "	5.5 "	8 "	2.7 "	3.7 "	1.7 "	8 "
19.....	3.7 "	8 "	4 "	8 "	1.7 "	3.5 "	1 "	7.7 "
20.....	4 "	7.5 "	5.5 "	8.5 "	2 "	2.5 "	2.5 "	8.5 "
21.....	4 "	8 "	5.7 "	8 "	2 "	3.5 "	1.7 "	7.5 "

Here again, at the beginning of each set of trials the stone was placed at the greatest distance at which the specimen went toward it on the preceding day, and then if necessary, it was brought closer until the nymph made a straight trip toward it, after which the distance was again gradually increased. The number of inches in the first column shows the distance at which the nymph made its first trip toward the stone each day, and that in the second column indicates the distance at which the trip was made at the end of 15 successful trials.

The experiments demonstrate the learning capacity of *H. interpunctata*. Not only did the insect show a tendency to learn in an environment free from disturbing factors, but, as can be seen from the results of the last experiments it learned to do greater and greater distances toward a stone against the rays of light. This is a fact of considerable interest, since these nymphs have a decided negative phototaxis. But, on the other hand they are very positive in their thigmotactic response, and the results seem to indicate beyond doubt that the satisfaction derived through attachment to the stone counteracted whatever reluctance the nymph may have experienced in going against the light.

FEAR REACTIONS

For this set of experiments I first took two large dishes and placed six vigorous specimens in each. Those in one dish were disturbed several times each day, while those in the other were left unmolested. Although the insects always show a tendency to gather into groups when placed in a dish of water, the tendency is greatly diminished in a few days. Such was the case in the undisturbed group. In the group undergoing disturbance this tendency was greatly augmented, apparently through fear. To overcome the grouping propensity the different individuals were put in separate dishes with particles of food too small to enable the insects to attach themselves, and to further obviate the discomfort occasioned through the lack of proper thigmotactic stimuli, the smooth glass bottoms of the dishes were covered with sand.

The room for this research was free from disturbance, and before investigations for the procuring of detailed data were attempted careful observations were made on the behavior of a set of nymphs with a view to detecting and overcoming any undesirable features of the environment. Then, six specimens in separate dishes were placed at one end of the room and subjected to four daily disturbances. In another part of the room were three individuals undergoing one disturbance each day; and some distance away were three more, disturbed but once in three days. In separate dishes scattered among these were six other individuals undergoing no disturbance whatever. Careful observations were made and detailed records of the behavior of all the specimens were kept.

Tables V and VI suffice briefly to show the results obtained with two of the specimens. Their behavior was characteristic of more than half of the 18 nymphs observed in this manner. The letters A, B, C, and D, designate the behavior displayed on the first, second, third, and fourth daily observations respectively. Each one of the three columns is intended for a particular form of behavior displayed by the nymphs, and the letters indicating the behavior manifested during the four daily observations are placed in the columns accordingly. Omission of a letter means that the nymph was not easily disturbed on that occasion. In the first column are recorded the cases of indications of fear only when the specimen was slightly disturbed with a dissecting needle. In column II are given the cases of signs of agitation when a hand was passed back and forth above the dish, and in the last column are indicated the signs of fear displayed by the specimen in the way of swimming about in an excited manner when I appeared before the enclosure. For example, on March 31, on the first observation, which was in the morning, specimen number one showed signs of fear only when the water near it was disturbed with a dissecting needle, and hence (A) was placed in the first column. On the second observation, which was at noon, the nymph began to swim about in an agitated manner when my hand was moved back and forth over the dish, thereby making a place for (B) in column two. On the third observation, which was about six o'clock in the afternoon, the insect displayed indications of fear when I appeared before the enclosure, and so (C) naturally fell in column three. On the last observation on that day, which was late at night, the nymph again showed signs of fear when a hand was moved back and forth over the dish, which caused (D) also to be in the second column.

A glance at either table V or VI at once suggests a curve representing the obvious intensification of the expression of fear. The letters indicative of signs of fear first appear in great numbers in the first column and later gradually make their appearance in columns two and three which represent the more obvious signs of agitation. All of the specimens, however, did not behave in this manner. Some, from the very beginning, would feign death when disturbed, while others reacted very favorably for several weeks and then, on account of their death-feigning propensity, which became more and more pronounced, little could be done with them.

TABLE V

Fear Reactions in *H. Interpunctata*, When Disturbed Four Times Each Day
Specimen I

Date	Indications of fear only when specimen was slightly disturbed with dissecting needle	Manifest signs of agitation when hand was passed back and forth above the dish	Signs of fear displayed in the way of swimming about in an excited manner when I appeared before the enclosure
March 20	C		
21	A C D		
22	B D		
23	A B C D		
24	A C D	B	
25	B D	A	
26	B C	A D	
27	B D	A	
28	A D	B C	
29	A B	C D	
30	B C	D	
31	A	B D	C
April 1	D	A C	B
2	B	A	C D
3	C	D	B
4		B C	A D
5	B		A C D
6		A D	B C
7		C	A B D
8		D	A B C
9	A	B	C D
10		B	A C D
11		B	A B C D
12		D	A B C

TABLE VI

Fear Reactions in *H. Interpunctata*, When Disturbed Four Times Each Day

Specimen II

Date	Indications of fear only when specimen was slightly disturbed with dissecting needle	Manifest signs of agitation when hand was passed back and forth above the dish	Signs of fear displayed in the way of swimming about in an excited manner when I appeared before the enclosure
March 20	B C		
21	A C		
22	A C D		
23	A B C D		
24	A B C D		
25	A C D		
26	A B D	C	
27	C D	A	B
28	A C D	B	
29	A B C D		
30	A B C D		
31	B C D	D	
April 1	A C D	B	
2	B C D		A
3	A B C D		
4	B D	A C	
5	B C D	A	
6	A B	C D	
7	A	B C D	
8	C	A D	B
9	A D	B C	
10	B	A D	C
11		B C D	A
12		A B C D	
13	B	A	C D
14	C	B D	A
15		A B C D	
16		A C D	B
17	D	B	A C
18	B	A C	D
19			A B C D
20		A B	C D
21		B	A C D
22		A D	B C
23		C	A B D
24	C	A	B D
25		B D	A C
26		D	A B C
27		A	B C D
28	A	C	B D

In table VII are given the results obtained with three of the individuals which were observed and disturbed once each day. In this case the numbers 1, 2, and 3, represent the three specimens, and the position of the numbers in any of the columns, like the position of the letters in the two previous tables, indicates the particular kind of behavior displayed by the specimen on that day. For example, on April 13, specimens one and two had to be slightly disturbed before they manifested signs of fear, while specimen number three on the same day showed signs of fear when I appeared before the dish.

TABLE VII

Fear Reactions in Three Specimens of *H. Interpunctata,* When Disturbed Once Each Day

Date	Indications of fear only when specimen was slightly disturbed with dissecting needle	Manifest signs of agitation when hand was passed back and forth above the dish	Signs of fear displayed in the way of swimming about in an excited manner when I appeared before the enclosure
1	1 2 3		
2	1 2 3		
3	1 2 3		
4	1 2 3		
5	1 3	2	
6	1 2 3		
7	2 3		
8	1 2		
9	1 2 3		
10	1 2 3		
11	2 3		
12	2 3		
13	1 2		3
14	1 2 3		
15	1 2		
16	1 3		
17	1 2 3		
18	2 3	1	
19	1 2 3		
20	2 3	1	
21	1 2		
22	1 2 3		
23	1 2 3		
24	1 2 3		
25	2 3		
26	1 2 3		
27	1 2	3	
28	2 3		
29	2	3	
30	1 3		2

These results seem to indicate that the nymphs formed few, if any, associations with pain resulting from a single daily disturbance. For the specimens disturbed once in three days there was even less evidence. Their behavior was almost identical with that of the undisturbed group, of which, during two months, one manifested signs of fear only twice; another three times; and four never stirred when I appeared or moved my hand over the dish. In the forms disturbed several times each day, however, even after making liberal allowance for accidental movements, there remains abundant evidence that the nymphs learned to associate my presence with discomfort.

FORMATION OF ASSOCIATIONS IN FEEDING

Training nymphs to come for food is a tedious task. This is due to many reasons, the most obvious of which appear to be: (1) The fact that in their natural environment they always feed attached to the under side of stones with their dorsal side downward; (2) they are moderate feeders and can go a long time—as long as two months—without food; (3) they are strong in their thigmotactic response, and great care must be exercised in dealing with the specimens in order to avoid fear reactions.

Six active specimens were selected and placed in separate dishes without food, in a compartment of the room employed in the experiments on fear. Daily observations were made as regards the position of the nymphs, and considerable time was spent each day in attempting to induce the different individuals to follow food to the end of the dish nearest to me.

At first a piece of alga was brought near the antennae of the nymphs with a pair of forceps. Within a few days practically all of the individuals ate the offered particle. A number of trials showed that only a small morsel of food was necessary each day to insure the proper response the succeeding day. As the nymphs would cling to the piece of alga with their fore claws I would hold on to the food with the forceps and turn the nymph so that it always faced me. This had to be done with considerable care in order not to dislodge the insect or interfere with its feeding. Then I would pull on the piece of food, endeavoring to induce the nymphs to follow. At first they would not move any great distance, but would plant themselves firmly and, if the pull was continued, the particle of food would either break or the nymphs would make an attempt to attach themselves to the forceps.

Later, I would hold a piece of alga near the head of the nymph and when it made an attempt to secure the food I would withdraw a little, in which case the insect would slowly crawl after it. This was a difficult task, for if the food was too far away the nymph would make no attempt to secure it, and again, when it was brought nearer, the insect would make a quick snap at it before I had time to withdraw it. For several weeks I was not able to entice the nymphs to follow the food any considerable distance before they would have their claws on it. However, when this was done the food was not taken away, but, by means of a gentle pull the insect was brought to the part of the dish nearest to me where it was allowed to feed, to a limited extent, undisturbed.

After experimenting with the insects in this manner daily for about four weeks I noticed that most of the specimens would frequently swim after the food when it was brought near them, and would often swim toward me when they happened to be at the further end of the dish when I made my appearance or moved my hand over them. Not all of the individuals which I experimented with reacted so favorably. Some, on account of their decidedly strong thigmotactic propensity, proved to be entirely intractable; while others absolutely refused to eat, and swam about very much agitated whenever a particle of food was brought near them. However, after two and a half months a large majority of the specimens plainly manifested, by means of swimming toward me or clawing against the side of the dish nearest to me, that my presence meant food to them.

From table VIII, on the feeding reactions of three of the specimens, it will be seen that not only did the insects show a growing tendency to remain in the feeding portion of their field, but that they learned to swim after the food. In column I, opposite the number of the day of the experiment, are indicated the positions of the three nymphs A, B, and C, respectively. The (+) sign indicates that the nymph occupied the end of the dish at which I always made my appearance and where the nymph was allowed to feed; the (—) sign indicates that the specimen was at the opposite end. For example, at the time of the observation on the third day of the experiment, specimen A was at the (+)

end of the dish or the end nearest to me, and specimens B and C were both at the (—) end, or the end farthest away from me.

Columns II, III, and IV show the gradual formation of associations in the three specimens. For instance, in column II are given the names of the specimens which have eaten some offered food, but would not follow it on the day opposite which the names are set; column III represents the cases of the same specimens when they followed the food as it was held near their mouth-parts with a pair of forceps; and in the last column are indicated the names of the same specimens which, when present in the (—) end of the dish would swim to the (+) end for food when I appeared or moved my hand over the dish, and the letters A, B, and C, which represent the three specimens, are placed in any one of the last three columns according to the kind of behavior manifested during the different daily observations. For example, on the fourth day of the experiment letters A, B, and C, occur in the second column which means that on that day all three of the specimens ate an offered particle of food, but would not follow it; nor did specimens B and C which were at the (—) end of the dish, swim to the (+) end when I appeared before them. On the twentieth day specimen A, which was at the (+) end of the dish, ate some food but did not care to follow it; specimen B, which was at the (—) end of the dish followed the food when it was held near it with a pair of forceps and hence B is placed in column III; and specimen C on the same date was also at the (—) end of the dish, but it swam toward the (+) end when I appeared and hence C was placed in column IV which is intended for that type of reaction.

It will be seen from the table that all three of the specimens indulged in food within a few days, but that not until the end of three weeks did all three swim after the food when it was offered to them with a pair of forceps. The most obvious signs of the formation of associations, recorded in the last column, did not appear until even later. The number of cases showing that form of behavior are noticeably less than those in the preceding columns, but it must be remembered that during the last days of the experiment the insects occupied the (+) end of their compartment the greater part of the time. Very frequently when the nymphs were at the (+) end at the time of my presence, the ends of the dish would be reversed, causing the specimen to be at the end farther away from me. This action was scarcely complete when the watchful nymphs would again be facing me and immediately proceeding on a rush to regain their former relative position. Such cases, however, are not recorded in the table.

TABLE VIII

Showing Gradual Formation of Associations in Three Specimens of *H. Interpunctata* in Their Feeding Reactions

Date	Column I						Column II			Column III			Column IV		
	Position of the nymphs in the dish. (+) indicates the end of the dish at which I always made my appearance and where the nymphs were allowed to feed; (—) indicates that the nymph was at the opposite end						Indications of the cases in which the nymphs have eaten an offered particle of food, but would not follow it			Record of the cases when the nymphs would follow a piece of food when it was held near their mouth parts			Record of the cases in which when the specimen was present at the (—) end of the dish it would swim to the (+) end when I appeared or moved a hand over it		
	Specimens														
	A		B		C										
1......		—	+			—			C						
2......		—		—	+			B							
3......	+			—		—	A								
4......	+			—		—	A	B	C						
5......		—	+		+		A					C			
6......		—	+		+		A								
7......	+			—		—		B							C
8......	+		+			—	A	B							
9......		—	+		+							C			
10......	+			—		—	A								
11......		—		—		—	A				B				
12......	+		+			—									
13......	+		+		+		A	B	C						
14......	+			—		—	A	B				C			
15......		—		—		—	A								C
16......		—	+		+										
17......	+			—	+		A	B				C			
18......		—		—		—									
19......		—	+		+						B	C			
20......	+			—		—	A				B				C
21......	+			—	+		A								
22......		—	+		+				C	A	B				
23......		—		—		—	A							B	C
24......	+		+		+		A				B	C			
25......		—	+		+			B		A					
26......		—	+			—				A	B	C			
27......	+			—		—				A	B				C
28......		—	+		+					A	B				
29......	+			—		—			C				A		
30......		—	+		+		A				B				

TABLE VIII—*Continued*

Date	Column I. Position of the nymphs in the dish. (+) indicates the end of the dish at which I always made my appearance and where the nymphs were allowed to feed; (—) indicates that the nymph was at the opposite end						Column II. Indications of the cases in which the nymphs have eaten an offered particle of food, but would not follow it			Column III. Record of the cases when the nymphs would follow a piece of food when it was held near their mouth parts			Column IV. Record of the cases in which when the specimen was present at the (—) end of the dish it would swim to the (+) end when I appeared or moved a hand over it		
	Specimens														
	A		B		C										
31......		—	+			—					B	C	A		
32......		—		—	+					A		C		B	
33......	+		+		+						B				
34......	+			—		—									C
35......	+			—	+				C	A					
36......		—		—	+							C		B	
37......	+			—	+					A	B	C			
38......	+		+			—					B	C			
39......		—	+		+							C	A		
40......	+			—	+					A		C		B	
41......	+			—		—				A	B				C
42......	+			—		—									C
43......	+		+		+		A							B	
44......		—	+		+				C		B		A		
45......		—	+		+						B	C	A		
46......	+			—	+					A		C		B	
47......	+		+			—		B		A		C			
48......		—	+		+					A		C			
49......	+		+		+						B		A		
50......	+			—		—								B	C
51......	+		+		+							C			
52......	+			—	+							C			
53......	+		+		+				C	A					
54......	+		+		+						B	C			
55......	+		+		+						B				
56......		—	+		+			B		A					
57......	+		+		+							C	A		
58......	+			—		—						C		B	
59......		—	+		+						B		A		
60......	+		+		+		A					C			
61......	+		+		+					A				B	
62......	+			—	+						B	C	A		
63......		—	+		+								A		C
64......		—	+			—				A				B	
65......	+			—	+					A		C			
66......		—	+		+						B				
67......	+		+		+							C		B	
68......	+			—	+					A					
69......	+														
70......	+	—											A		
71......	+	—													
72......	+														
Average.	43	29	40	28	44	24									

Should a line be drawn through the parts of the table where the letters appear most abundantly we should have a gradual curve representing the reinforcement of the reactions to a given stimulus as a result of the formation of associations. The behavior of the three nymphs represented in the foregoing table is characteristic of many other nymphs experimented with. To check these results, daily observations were made in regard to the position and behavior of another set of individuals which had a food supply scattered in small particles at the bottoms of their dishes. Their records show that they exercised no choice between the two ends of the dish, and that they never swam toward me as did those in the training experiments. The trained nymphs no doubt formed an association.

Not infrequently during the work with *H. interpunctata* nymphs did I observe specimens which behaved in an uncommon manner. The most interesting individual which attracted my attention was in the midst of a large number of newly collected specimens in a dish of water. Its peculiar conduct led me to observe it carefully, when I noticed that it skipped about among its fellows, lashing its setae and occasionally making a pass at one of the other nymphs with its front limb like a playful kitten.

It was placed in a separate dish and taken to my room where it was observed and trained daily. After a few days I noticed that its tendency to remain on the upper surface of the stone, whenever the stone was inverted, became more and more pronounced. The nymph was brought to the upper surface in that manner several times each day and on every occasion it was offered a particle of alga. So long as it was treated gently it would remain in that position for a considerable time, but, the moment it was dealt with roughly it would conceal itself under the stone. Great care was taken not to frighten the insect, and within a few weeks it would remain on the upper surface of the rock for hours at a time facing me. Whenever I noticed that the nymph had gone under I would turn the rock over. In about two weeks more, it had almost completely inhibited its shyness and would follow a particle of food to any part of the stone. Then I removed the much coveted stone and placed in the dish a stone bearing a finger-like projection about three inches long. The stone was so submerged that the extreme point of the process projected about half an inch above the surface of the water, pointing toward me. The insect at this stage would readily follow a piece of food to the projection, which became its regular feeding place. At the end of about two months the insect spent practically all of its time, during my presence, perched up near the point of the projection with its head just below the surface of the water. The time and clearness of the day seemed to play no important part on its behavior. Even at night when the electric light was directly over the table on which the insect was located, it would not desert its favorite resting place. Almost invariably, when I had left the room the nymph would soon retire to the under side of the stone. All that was necessary, at this time, to bring the specimen up when it had disappeared from sight was to slightly jar the dish or the table on which the dish was located, and the insect would quickly come up to the upper side of the rock and make directly for its feeding place. Later, very frequently as I stepped into the room I caught the insect in the act of coming out from its hiding-place and halting at the end of the projection ready to receive its usual reward.

Sometimes, when I had been gone from the room all day and did not feed the nymph shortly after my appearance, but simply moved my hand over it, the insect would slowly crawl up on the point of the rock extending out of the water. However, it usually exposed only its head and thorax to the air and apparently exercised considerable care not to expose the abdomen, which bears the tracheal gills. Although I anticipated extended experiments with this entertaining individual, all was brought to a close by its metamorphosis. Nothing more could be done with it in the adult stage, as the Ephemeridae never take food after they emerge, and live but a few days at most. Here again, we have obvious signs of the formation of associations.

Sondheim[1] accidentally made some observations and later performed some experiments with a dragon-fly larva, *Aeschna grandis L.*, and obtained practically similar results during a much shorter time and with far less effort. This is probably due to the fact that the natural habits of *A. grandis* are much different from those of *H. interpunctata*. However, she obtained such results with only a single specimen and although attempts were made to investigate the learning capacity of another larva of the same family, she was unable to get any further evidence of the formation of associations. "Seit einigen Wochen befindet sich wieder eine Libellenlarve in meinem Aquarium, doch habe ich bei ihr noch nicht viel Erfolg mit meinen Fütterungsversuchen gehabt."

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[1] Sondheim, Maria. Wahrnehmungsvermögen einer Libellenlarve. Biologisches Centralblatt, Bd. 21, S. 317-319. 1901.

CONCLUSION

In conclusion it might be said that we have three types of experiments with large numbers of *H. interpunctata* nymphs which clearly show the power of formation of associations by three distinct processes. In case of the investigations on the reactions to objects it was found that the insect gradually increased the distance at which it responded positively to objects in an evenly lighted environment. In another set of experiments it gradually inhibited its usual negative response to light and proportionately reinforced its reaction to an object against the rays of light. It is also important to note the fact that this inhibition in phototactic response was only temporary; it occurred only when the object was present, for whenever the object was removed the nymph would manifest its usual dislike for the bright light. In case of the experimental work on the manifestations of fear through previous experience we have results which can hardly be interpreted in any other way except as a clear example of the associations of my movements with pain, for why should the disturbed specimens show such obvious signs of agitation when the undisturbed group evinced no indications of fear under otherwise practically identical conditions. Lastly, we have results which seem to indicate quite conclusively that the insects associated my presence with food. These results, again, become more convincing when compared with the unvarying individual records of the group not so trained. The foregoing conclusions are not based on the results tabulated in this paper alone, but on the results of a large number of similar experiments performed with many large sets of specimens and continued for two years during the various seasons.

My thanks are due to Professor S. J. Holmes for his kind suggestions and criticisms in preparing this paper.

THE NORMAL ACTIVITY OF THE ALBINO RAT FROM BIRTH TO NATURAL DEATH, ITS RATE OF GROWTH AND THE DURATION OF LIFE

JAMES ROLLIN SLONAKER
From the Physiological Laboratory of Stanford University

Fourteen figures

INTRODUCTION

The purpose of this experiment was to ascertain the normal activity of the albino rat from birth to death due to old age, its rate of growth, and the duration of life. From previous experiments [1] it was seen that to reach these results a number of rats of the same age and parentage should be used. To procure such material female rats of the same litter were segregated and mated to the same male. After a number of trials lasting over a year two sister rats, having been mated to the same male, gave birth to an equal number of young just a day apart. It was of great importance to have the young rats as nearly alike as possible. Previous observations have shown that prenatal and postnatal conditions greatly affect the rate of growth and activity.[4] For example, when the conditions of parentage were as nearly alike as possible the young of a litter of three would have a decided advantage over those of a litter of eight. That is, the individuals of the smaller litter were not only a trifle larger at birth but also grew much faster due to the fact that each received more nourishment than was possible in the larger litter. Since the young had to be nourished by the mother until they were old enough to wean it was deemed very important that not only should the mothers be as nearly alike and related as possible, but that each litter should be of the same number and age. With this material in hand conditions were as nearly ideal as it was possible to obtain.

In selecting the young for the experiment an equal number of each sex should be used, but owing to the fact that the external sexual characters are not easily distinguishable at the weaning time it was not possible to secure this ideal condition.

Eight young were selected from the two litters. They were arranged and designated in the following manner: 1 and 2 from litter A; 3 and 4 from litter B; 9 and 10 from A; and 11 and 12 from B. Numbers 1 to 4 inclusive were placed in revolving cages to ascertain their normal activity as manifested by voluntary running. The remaining four (9, 10, 11 and 12) were used for controls and were placed in stationary cages in which rats are usually kept. In every case each rat was in a cage by itself. This was its home during its life from which it was removed only for a time sufficient to weigh the animal and clean its cage. It was later ascertained that in this selection six males and two females had been chosen. Fortunately three males and one female comprised each of these two groups of four. Numbers 1, 2, 4, 9, 10 and 12 were males; 3 and 11 were females.

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[1] Slonaker, J. R. The normal activity of the white rat at different ages. *Jour. Comp. Neurol. and Psychol.*, 1907, vol. 17, pp. 342-359.

The apparatus used for recording the activity has been previously described.[2] In brief it consisted of a large cylindrical cage which revolved on a stationary axle. On the axle was fastened the nest box, the food box and the water pan. The cage revolved only when the rat ran. The number of revolutions were automatically registered and at the same time each revolution was automatically marked on paper kept moving by a continuous roll kymograph. By the first device the exact number of revolutions in a given time or the total number could be readily ascertained for each rat. By the second contrivance the distribution and length of periods of activity and rest during the day, night and lifetime could be consulted and compared at any time. This graphic representation of the activity has been of great assistance as only by this means do the characteristics of the activity due to different ages appear.

Only that phase of the activity of the rat manifested in voluntary running has been considered in this research. It was impossible with this apparatus to record any of the other activities. However, numerous observations of the activities of the rat readily convinces one of the fact that though running is only one phase of activity it goes hand in hand with and is directly proportional to the other activities. That is, if a rat is in general very active at a given age it does a great deal of voluntary running which has an apparent definite ratio to the other activities. For this reason this one phase of the normal daily activity has been taken as an indicator of all the activities of an individual.

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[2] Slonaker, J. R. Description of an apparatus for recording the activity of small mammals. *The Anat. Record,* June, 1908, vol. 2, pp. 116-122.

Fig. 1

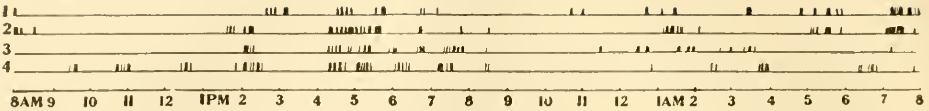


Fig. 2

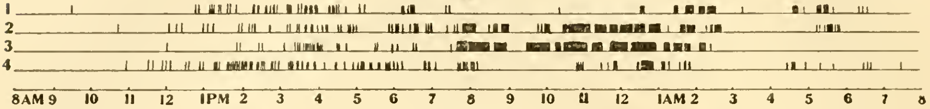


Fig. 3

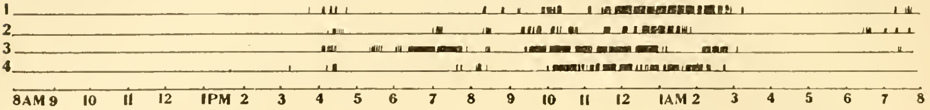


Fig. 4

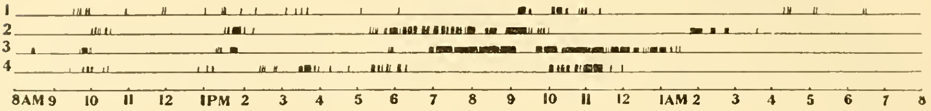


Fig. 5

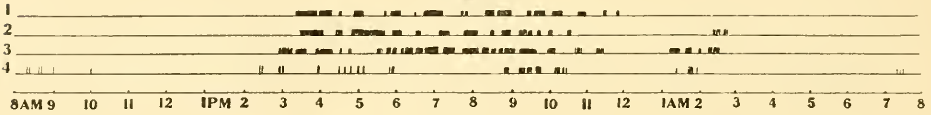


Fig. 6

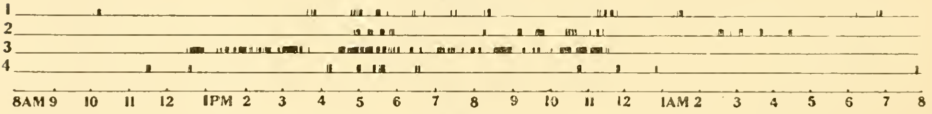


Fig. 7

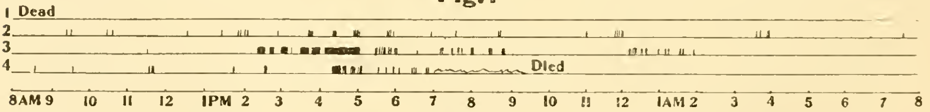


Fig. 8

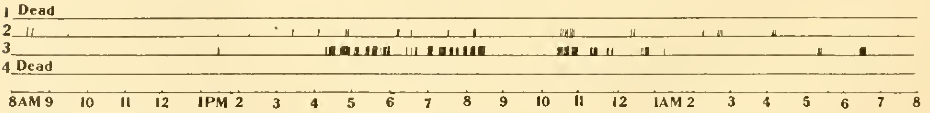


Fig. 9

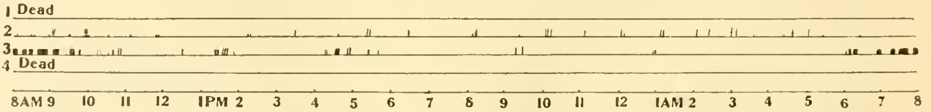
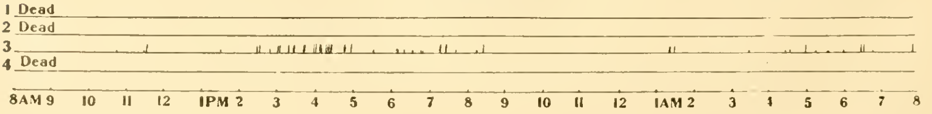


Fig. 10



In connection with their activity it was very easy to determine the rate of growth and final decline due to old age. Careful weighings were taken at sufficient intervals to indicate this.

DISTRIBUTION OF THE DAILY ACTIVITY

The young were taken from their mothers when they were 28 days old. They were carefully weighed and placed in their individual cages as has already been noted. Hereafter they will be designated by the numeral given each.

When first introduced into its cage each rat showed a marked degree of curiosity. It examined every corner and object within the inclosure, using mainly the sense of smell assisted by the sense of touch and to a still less degree that of sight. After about an hour of such investigation most of those in the revolving cages had found their way to the nest boxes in which they remained for a time. The graphic record of their activity (Fig. 1) shows that their periods of activity and rest were numerous. The activity would consist of a few turns of the cage then rest soon to be followed by another short run. In only a few cases did an individual keep up the activity longer than a few minutes at a time. At this age the periods of activity were not confined to any special part of the day, but were rather uniformly distributed throughout the 24 hours. Coupled with this activity manifested in running was that accompanying investigation by the animal as already noted. After a few days, however, they became familiar with their surroundings and appeared to be perfectly at home. Their activity then was characterized by their running. At intervals they would frisk and jump about in play, a characteristic of most young animals. As they grew older this playfulness became gradually less and less and was replaced by more uniform and regular running.

By consulting Figs. 1–10, which are graphic records of the

Figure 1. Graphic record as recorded by the kymograph of the activity when the rats were 32 days old and 4 days after they had been weaned and placed in the revolving cages.
Figure 2. Graphic record of activity at the age of 6 months.
Figure 3. Graphic record of activity at the age of 11 months.
Figure 4. Graphic record of activity at the age of 16 months.
Figure 5. Graphic record of activity at the age of 21 months.
Figure 6. Graphic record of activity at the age of 25 months.
Figure 7. Graphic record of activity at the age of 26 months.
Figure 8. Graphic record of activity at the age of 28 months.
Figure 9. Graphic record of activity at the age of 31 months.
Figure 10. Graphic record of activity at the age of 34 months.

activity at the ages indicated, one can get a good idea of the nature of their activity. Fig. 1 represents their activity at the age of 32 days, or four days after they had been weaned and placed in the revolving cages. As can be readily seen the periods of continuous running are of very short duration. Also the periods of activity and rest have no definite arrangement with respect to the hours of the day. The longest period of continuous rest is about six hours and the longest period of continuous activity lasts but a few minutes. This result was no doubt due to the fact that at this age the young are accustomed to feeding at frequent intervals. Since the rats could not go from their nests to the food without jumping on to the revolving cage they left their graphic records each time they passed from the nest box. But one must not infer that each graphic record represents a feeding time. Numerous observations proved that they revolved the cages many times without going near their food.

Fig. 2 is the graphic record at the age of six months. This shows some marked changes. Though they still show a more or less restless disposition throughout the 24 hours the main part of their activity is confined to the hours between 7 p. m. and 3 a. m. During these hours they ran almost continuously for periods of 30 or 40 minutes at a time. The records are slightly misleading in this respect. The kymograph paper moved so slowly that intervals of two or three minutes could occur without a break or space showing in the record. A solid block on the record, therefore, does not necessarily mean that a rat has run continuously without a single stop for the time indicated. The longest period of continuous running without a single stop which was observed was between four and five minutes. During this time they would often turn the cage at the rate of a hundred or more revolutions per minute. After a rest of a minute or so they would again run for a number of minutes. The record of activity would thus appear to be continuous so long as the interval of rest was not longer than about two minutes.

The very intermittent and irregular activity seen from 11 a. m. to 7 p. m. is easily explained. They were accustomed to being fed between 3 and 5 p. m. At this age they were more or less restless for an hour or so before feeding. After feeding they carried all they could conveniently into their nest boxes.

On this day they were fed at 3:20 p. m. It is noticeable that at this age they had become much more regular in their habits and, having become thoroughly accustomed to their surroundings, did not manifest the investigating nature which was so prominent when they were younger. The playfulness which was so conspicuous at an early age was less marked.

In Fig. 3 is seen the record of their activity at the age of 11 months. It is now noticed that the activity begins mainly with the feeding time (4 o'clock) and is practically complete at 4 a. m. Activity and rest are thus divided into two almost equal periods of 12 hours each. The period of activity being confined mainly to early twilight and night and that of rest to day time. Playfulness in action was not manifest at this age.

At the age of 16 months very little change is to be noted in the general activity (Fig. 4). A week previous to this record the feeding time was changed to the morning to determine what effect this would have on the distribution of their activity. As is readily seen the usual random running found to generally follow the feeding shifts to the morning, but the great bulk of voluntary running occurs during the period of darkness and is apparently in no way affected by the change in the time of feeding. In other words the albino rat, like the wild Norway rat, is nocturnal in its habits.

Fig. 5 is the record of their activity when they were 21 months old. At this age the rats were almost wholly inactive from 3 a. m. to 3 p. m. Their day was thus divided into two equal periods of 12 hours each. Number 4 is beginning to show the effects of old age as the periods of activity are of relatively short duration and are separated by relatively long periods of rest. Also this rat, though it has still done the greatest amount of running during the night, has been more or less active throughout the entire day. This is especially noticed in the records of all at older ages. In other words, as the rats reach old age their activity resembles more and more that of the very young rat in distribution, but is less in amount.

At the age of 25 months a marked diminution in the amount of running is noticed (Fig. 6). It is now very obvious that the female (No. 3) is much more active than the males (Nos. 1, 2, and 4). The activity of each of the males is very similar and shows the characteristic activity of rats in old age. Their movements were very slow and feeble having lost all the alertness and quickness common to their youth. The record of No. 1 represents its activity just two days prior to death due to old age. During the last day of its life this rat turned its cage 94 revolutions which was a small amount compared with over 6,000 accomplished in the prime of its life.

Fig. 7 represents the activity at the age of 26 months. The two remaining males, Nos. 2 and 4, show the characteristics of old age, while the female No. 3 still shows the activity characteristic of middle life. The record for No. 4 shows the last day's work of this rat which consisted of 48 revolutions. It also shows its final death struggles which began at 7 p. m. and ended soon after 9.

The remaining figures, 8, 9 and 10, show the effect of extreme old age. They represent the ages of 28, 31 and 34 months respectively. Fig. 9 shows the record of No. 2 just two days previous to its death. It is quite similar in its appearance to that of No. 3 in Fig. 10, which is the record of this rat just two days prior to its death at an age of almost 34 months.

From the observations of the records (Figs. 1 to 10) of any one rat, from the first to its death, the following points may be easily seen:

1. The daily activity increases with the advance in age until a certain age is reached, after which there is a gradual reduction till death occurs.

2. Distribution of the activity. In youth it is rather uniformly distributed over the entire 24 hours. With maturity activity becomes concentrated into one period and rest into another. In extreme old age the activity becomes scattered and resembles that of the very young.

3. During the prime of life the albino rat shows almost continuous activity for periods of as much as two hours at a time (Fig. 4, No. 3).

4. The albino rat is nocturnal in its habits.

5. This female is much more active than any of the males.

THE AMOUNT OF DAILY ACTIVITY

We have just considered the character of the activity of the rat at different ages in regard to its distribution during the entire day. Let us now consider the actual amount of running activity exhibited at the different ages. Owing to the device which recorded the number of revolutions it is very easy to estimate the distance run each day during the life time. Since the change is so gradual and a computation of each day would be so voluminous I have considered computations made once each month sufficient to represent correctly the change in the

TABLE I

AVERAGE DAILY REVOLUTIONS AT AGES INDICATED

Age in Months	Rat No. 1 (Male)	Rat No. 2 (Male)	Rat No. 3 (Female)	Rat No. 4 (Male)
1	348	1002	620	448
2	1200	1124	2118	1336
3	790	886	1956	1832
4	5028	5850	5782	6620
5	1206	3930	7280	834
6	540	1644	8482	540
7	1307	3144	10185	3574
8	2084	3274	9518	4276
9	6002	7574	8930	2836
10	2616	4398	8260	2882
11	3620	2096	8640	2310
12	2186	4258	8902	2616
13	3860	7066	11044	3084
14	3860	6374	10992	3004
15	1428	4768	8198	2556
16	1006	2642	13002	1364
17	2684	4776	5982	1998
18	744	2364	9732	1512
19	996	2476	8312	808
20	806	2056	9658	720
21	690	1520	3210	482
22	336	1810	5468	290
23	1696	1680	3188	456
24	380	1352	3100	220
25	94*	1466	4612	196
26	....	188	2538	48*
27	....	160	1968	....
28	....	180	5171	....
29	....	72	2424	....
30	....	56	2832	....
31	....	28*	1706	....
32	....	....	2312	....
33	....	....	488	....
34	....	....	435	....
35	....	....	60*	....

* The number of revolutions made during the 24 hours previous to the death of the animal.

amount of running activity during the lifetime. In order that the figures representing the activity at a given age may not represent a single day's run, but an average day's run at that age the following scheme was used. The number of revolutions for the five days preceding and the five days succeeding the day representing the age desired were taken and from these an average day's run was estimated. This eliminates the possible effect of food, temperature, or other conditions which might cause a sudden slump or increase in the activity of a given day.

Table I represents such averages and shows the average number of revolutions made each day by each rat at the ages indicated. Consulting this table it is readily seen that in each case there is a rapid increase in average daily running. But instead of its being a uniform increase it was rhythmic in character. There was a rapid increase in the amount of activity for a month or so after which there was a decrease. This was followed again by another increase and decrease. The rhythmic effect is best shown in Fig. 11. These curves show the average daily running for each rat at the ages indicated. In other words it is the data of Table I put in the form of curves. The difference in the average daily activity of the males and the females is very noticeable. It is also interesting to note that the rhythmic increase and decrease of activity of each of the males accords very closely as to time, while that of the female does not correspond so closely. So far I am not sure of the causes which underlie this rhythmic variation. These changes correspond more or less to the fluctuations in the weight of the rats as shown in Fig. 13. The causes for the changes in weight will be discussed later under the topic of growth. I will, however, state that following an increase in the amount of protein food given there was an increase in weight accompanied generally by a greater or less increase in activity. But as the results are not always constant there must be something else which influences and causes these rhythmic fluctuations. If the changes in activity of the female corresponded closely to those of the males or as closely as the males do among themselves I would consider they might be due to the periods of heat. At such times there is a tendency in both sexes when near each other to excessive running. The fact that No. 2 and No. 4 were situated one on each side of No. 3 and that these two males did more running

Fig. 11

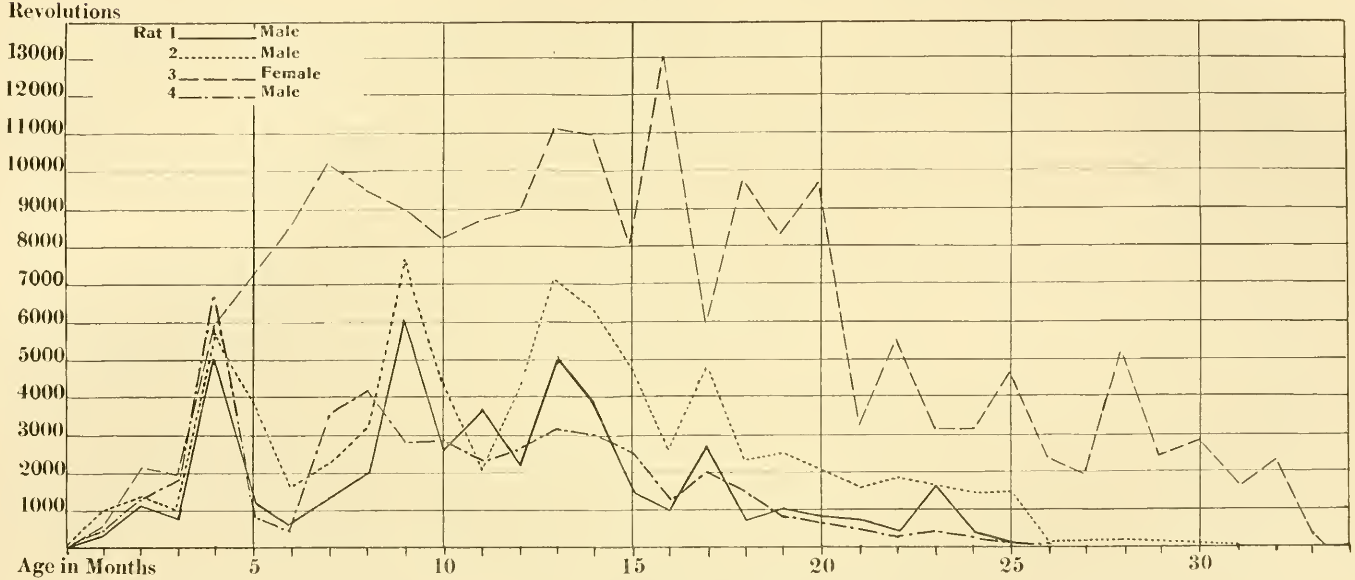


FIGURE 11. Curves representing the average daily activity of each rat at the ages indicated.

than No. 1, which was farthest away, would seem to indicate that this supposed condition may have had its influence. Though this supposition may be true, owing to the differences in the curves of the two sexes, one is not justified in placing very much credence in it. Other changes in their environment no doubt are responsible to some extent for these fluctuations. This will be discussed in more detail later.

By comparing the figures in Table I and the curves in Fig. 11 a marked difference is observed in the average daily activity of the two sexes. For the first four months there is not much difference, but after this age the female excels the males to a great extent, and is never equaled by them again. The greatest average daily run as shown in this table was accomplished by the female and consisted of 13002 revolutions, or a distance of 11.2 miles. This same rat has a record of 14.2 miles during 24 consecutive hours. The greatest average daily run of any of the males was 7574 revolutions. which was equivalent to 6.6 miles. The greatest average run for the four rats was at the age of nine months. At the age of four months a uniform and very marked rise in the curves is noticed, but as each rat with the exception of No. 4 surpasses this amount of activity at a later age this cannot be considered the age of greatest activity. It is true, however, that the average for the males alone is greater at this age than at any later time but it exceeds by only a few revolutions. Hypothetical curves representing the mean between the two extremes of activity for males and females would show the greatest amount of work for the males at about the tenth month and for the female at about the age of 12½ months. Owing to the fact that there was but one female too much dependence cannot be placed on this result since it was not an average. It is interesting to note, however, that the ratio of the age of greatest activity (10 months) of the males to the average age of their death (27 months) is 1:2.7 which is practically the same as that for the female (12.5 months: 33.8 months) 1:2.7+. If this ratio can be depended upon, a normal white rat would voluntarily do the greatest amount of daily work at an age a little older than one-third that of its whole normal life. It would be exceedingly interesting to know if this ratio would apply to other animals.

TOTAL AMOUNT OF ACTIVITY DURING LIFE

Table II represents the total amount of activity of each rat at the different ages as indicated by the number of revolutions. In order that this data may be more comprehensible not only the number of revolutions is given but also the equivalent distance in miles. This same data is represented in the form

TABLE II

Total Number of Revolutions and Distance Run at the Ages Indicated

Age in Months	Rat No. 1 (Male)		Rat No. 2 (Male)		Rat No. 3 (Female)		Rat No. 4 (Male)	
	Miles Run	Number Revolutions	Miles Run	Number Revolutions	Miles Run	Number Revolutions	Miles Run	Number Revolutions
1.......	1	1200	2.6	3050	1.1	1300	1.7	2060
2.......	16	18850	22	26140	19	23010	19.6	23390
3.......	68	79750	77	91130	95	111320	100	118150
4.......	121	148460	160	189620	170	201730	210	248780
5.......	211	248990	273	322420	336	406780	346	409250
6.......	278	329900	373	438020	572	672070	495	508500
7.......	341	403070	434	523320	882	1041330	550	656470
8.......	414	487130	551	650490	1181	1388260	658	777640
9.......	487	576360	663	786660	1435	1678890	740	870400
10.......	612	722880	832	982210	1710	2028190	810	952860
11.......	691	817000	915	1082000	1980	2332260	876	1039650
12.......	749	883120	1023	1215190	2230	2647640	945	1104240
13.......	812	961360	1155	1378680	2490	2938160	1036	1218620
14.......	900	1068300	1318	1548570	2780	3286540	1115	1319100
15.......	986	1165990	1390	1666150	3020	3559830	1168	1389940
16.......	1020	1206980	1490	1760900	3230	3820590	1220	1455930
17.......	1080	1271700	1560	1849340	3460	4091400	1270	1488930
18.......	1110	1309080	1665	1975140	3650	4318780	1298	1529400
19.......	1125	1339080	1747	2072520	3890	4599500	1325	1571080
20.......	1165	1382130	1850	2146720	4125	4867910	1347	1588710
21.......	1188	1400500	1862	2203380	4280	5076100	1358	1601920
22.......	1210	1429570	1920	2277120	4475	5290770	1362	1613700
23.......	1225	1457500	1980	2334160	4620	5462560	1380	1624420
24.......	1244	1469340	2025	2380260	4720	5573480	1385	1631780
25.......	1265	1486250	2050	2423250	4810	5679360	1389	1639060
26.......			2078	2447180	4900	5796630	1391	1642100
27.......			2090	2456190	5000	5909860		
28.......			2093	2469110	5100	6018140		
29.......			2095.6	2462520	5180	6118320		
30.......			2097	2463950	5250	6202570		
31.......			2097.7	2464770	5300	6259910		
32.......			2098	2465130	5350	6353920		
33.......					5430	6414900		
34.......					5447	6427500		

Fig.12

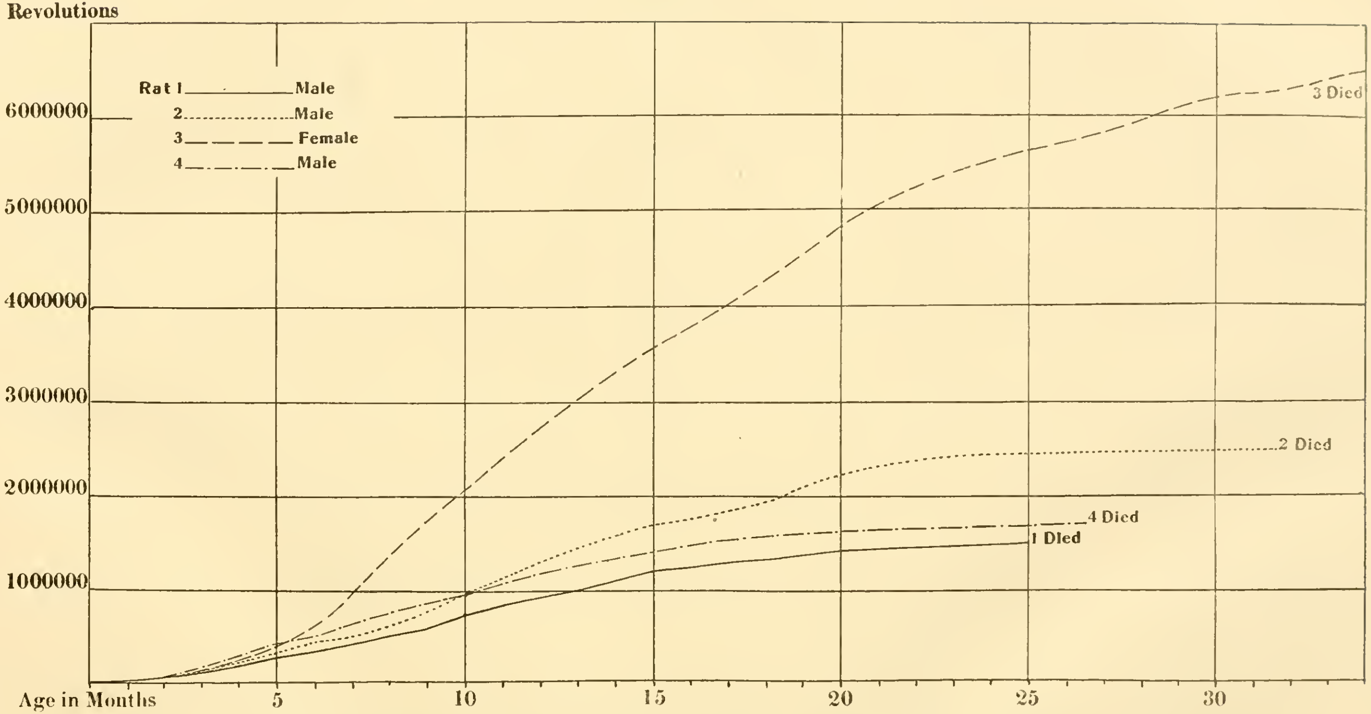


FIGURE 12. Curves representing the total amount of work and the rate it was performed of each rat during its lifetime.

of curves in Fig. 12. The most noticeable thing in this figure is the great difference existing between the males and the female. They all kept fairly close together until the fifth month of age. After this the female, No. 3, rapidly surpassed the males in total amount and ended her life with a total run of 5447 miles. This distance is nearly three and one-half times as great as the average distance run by the males. In other words the ratio of total activity of the male to the female was 1:3.44.

It seems almost incredible that a rat would run 5447 miles during its lifetime. For this rat it was an average run of a little less than 5.5 miles each day. During its very young days and its old age it did not run this distance each day. Throughout the prime of its life, however, it was no uncommon thing for it to run 10 or 12 miles a day. The males averaged less than two miles each day during their lifetime. Since the main bulk of the activity was confined to the 12 hours from 3 p. m. to 3 a. m. we can see that the rate of running for the female averaged almost one-half mile each hour and for the males a little less than one-sixth of a mile per hour of activity during their lifetime.

We have already said that the greatest amount of daily activity was accomplished by the males at the age of 10 months and by the females at the age of 12.5 months. What portion of the total amount of work accomplished during their lifetime had been performed at these ages? Table III shows not only this but also the ages and the percentage equivalence of total

TABLE III

Percentage of Total Life Span Required to Do Certain Fractional Parts of the Total Life's Work and the Age in Months at Which These Amounts of Work Are Completed.

Fractional part of total amount of life's work		1/8	2/8	3/8	4/8	5/8	6/8	7/8
Males (Average for 3)	Percentage of life equivalent to age	16.1	21.4	32.1	36.4	46.4	53.8	67.9
	Age in months....	4.5	6	9	10.2	13	15	18
Female (Average for 1)	Percentage of life equivalent to age	19.1	26.1	32.3	41	48.8	53.8	72.6
	Age in months....	6.5	8.9	11.1	13.8	16.6	19.9	24.7

lifetime at which certain arbitrary fractional parts of the total amount of work was performed. The males had accomplished one-half of their total activity at the age of 10 months, which represented 36.4 per cent of the whole life time. When the female had performed half of its total activity it had reached the age of 13.8 months or 41 per cent of its lifetime. This was at a trifle later age than when this rat accomplished its greatest amount of daily activity.

This table shows us some very interesting things. It shows that the males turn off their work at an earlier age in life, representing a lower percentage of lifetime than the female. This is true in the case of each of the arbitrary fractional parts of their total work which has been selected. One-half of the total work has been accomplished when but 36.4 per cent of the male and 41 per cent of the female's life has been passed. When seven-eighths of the total work was done they still had about 30 per cent of their lives to live. In other words only one-eighth of their work was still left for them to do during this comparatively long time. This shows a marked contrast in the activity of youth and old age. In the young males for example it required but 16.1 per cent of their lifetime to do one-eighth of their life work, while in old age it required 32.1 per cent of their lifespan to perform the same amount. At this latter percentage of lifespan they had accomplished in their youth three-eighths of the total work.

Since these animals were in no way compelled to work but did it of their own volition we can readily conclude that we might expect a greater amount of work during youth up to middle age than in the latter half of life. I think one is justified in saying also that such animals could be driven to do a much greater amount of work during the first half of their life than the last half. From general observations and these data I believe one is warranted in saying that similar results would be obtained from other animals including man.

THE RATE OF GROWTH AND LENGTH OF LIFE

Owing to the fact that the mothers sometimes destroy their young when they are disturbed no weights were made of the young until they were weaned. Other investigators have studied the growth of other animals from birth. They have come to the general conclusion that the greatest percentage of gain in weight is made soon after birth and this percentage of increase diminishes rapidly during the first month of life.

Minot [3] has found that in the growth of rabbits the average daily gain up to five days of age is 17.6 % for the males and 16% for the females. At the age of from 25 to 30 days the gain had dropped for the males to 5.3% and for the females to 6.8%. In the case of the growth of the chicken the average daily increase drops from about 9% on the fifth day to about 5% on the thirtieth day. He found a somewhat similar condition to obtain in the guineapig.[4]

Donaldson [5] in speaking of the growth of man says "* * * from birth to the first year there has been an increase in weight of 240% for the males, and 190% in the females. But this rate diminishes during the succeeding years with astonishing rapidity."

Miller [6] in a paper on the Norway rat has found that the male is 52 mm. long and the female is 49 mm. at birth. The average weight of both sexes at birth was found to be 6.4 grams. There was a gain of from 1/7 to 1/10 in 24 hours. By the seventh day they had doubled their weight; by the eleventh day they had trebled it; by the fifteenth, they had quadrupled it; and by the seventieth day they had reached an average weight of 115 grams, or 18 times their weight at birth. This is somewhat heavier than the average albino rat, but no doubt the same relative increase would occur during the first month. The average weight of the albino rat at the age of 70 days, according to Table IV, is 84 grams.

In order to determine the growth the rats were weighed before feeding at intervals of about two weeks. This was often enough to show the changes in weight during the life time. It was found that the changes in weight were fairly gradual and that weights taken at greater intervals would be sufficient. Accordingly the weights which were made about a month apart were selected in constructing Table IV. Figs. 13 and 14 were

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[3] Minot, C. S. The problem of age, growth and death. 1907, pp. 254 and 258.

[4] Minot, C. S. Senescence and rejuvenation. First paper—On the weight of guinea pigs. *Jour.* of *Physiol.*, vol. 12, 1891, pp. 98-153.

[5] Donaldson, H. H. The growth of the brain, p. 62.

[6] Miller, Newton. Reproduction in the brown rat. (*Mus. Norwegicus*). Amer. Nat. 1911. vol. 45, pp. 623-635.

TABLE IV

Showing Rate of Growth as Indicated by the Increase in Weight. The Maximum Weight Attained is in Bold Figures. Weights are in Grams.

Age		Exercised Rats in Revolving Cages				Control Rats in Stationary Cages			
Mos.	Days	No. 1 Male	No. 2 Male	No. 3 Female	No. 4 Male	No. 9 Male	No. 10 Male	No. 11 Female	No. 12 Male
	30	42	43	37	39	34	45	39	45
2	11	79	89	76	68	84	100	84	92
3	5	107	123	107	99	146	157	124	141
4	5	132	143	123	128	183	200	140	178
5	3	145	165	136	145	214	225	154	205
6	3	170	179	141	169	241	249	165	242
7	4	179	195	152	185	265	262	174	265
8	13	201	219	158	205	282	283	181	292
9	13	206	218	161	222	288	289	180	298
10	11	215	245	163	221	291	301	184	308
11	9	217	248	161	226	296	304	**200**	315
12	13	226	247	159	230	308	322	199	309
13	2	232	265	170	238	**310**	**331**	135*	**319**
14	3	234	259	168	246	291	295		296
15	1	231	254	164	240	291	295		294
16	17	242	**272**	168	252	286	300		298
17	1	249	268	**176**	257	289	298		304
18	4	**252**	270	165	258	302	310		313
19	22	245	269	159	**261**	297	293		310
20	24	240	244	153	249	290	282		291
21	15	245	262	161	250	290	284		290
22	12	244	270	163	254	295	280		294
23	7	235	261	159	242	288	270		292
24	10	235	269	163	248	292	283		294
25	6	214	271	155	233	290	283		284
26	18		259	148	215	278	272		297
27	18		252	154		287	275		294
28	7		256	139		284	278		294
29	7		251	147		293	270		302
30	15		263	157		288	268		300
31	7		264	163		288	263		304
32	5		206	151		279	269		302
32	20			145		271	258		285
33	9			159		264	252		280
33	26			128		259	251		277
34	26					264	249		280
35	9					247	225		268
36	2						242		277
37	10						229		269
37	25						227		259
38	18						226		238
39	6						225		230
40	6						220		225
40	24						210		203
41	11						203		
44	16						185		
45	12						170		

* This rat was killed because of sickness.

constructed from the data in this table and represent the curves of growth of each of the exercised rats and the control rats respectively.

By comparing Figs. 13 and 14 it is readily perceived that the control rats reach a heavier weight and also live longer than those in the revolving cages. This is especially noticeable when the males are compared. The exercised and control female rats can be compared only during the first 11 months. About this time No. 11 showed signs of illness. At the age of 13 months the malady had developed into a disease common to the white rat. It had been kept in hopes that it would recover, but as it was rapidly declining and there was great

Fig.13

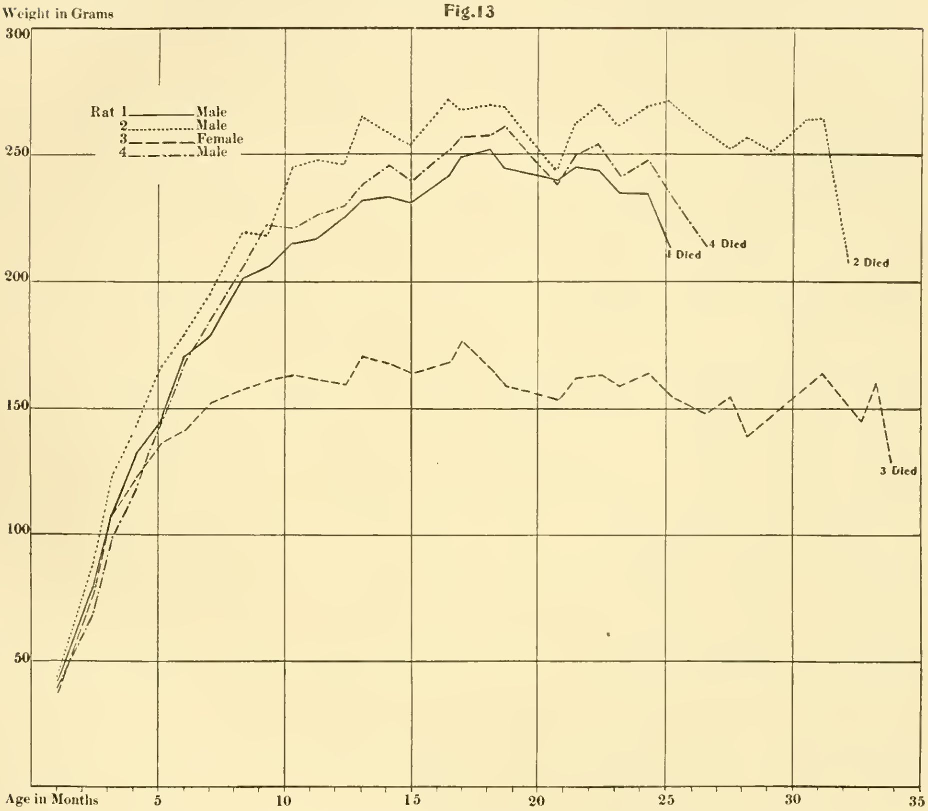

FIGURE 13. Curves representing the growth and decline of the exercised rats during their lifetime.

danger of the other rats becoming infected this one was killed. They can, however, I think be relied upon to the eleventh month as it seemed perfectly normal in every respect. Whether it would have reached a heavier weight than 200 grams can not be predicted, but judging from the growth of the others I feel convinced that it would have grown to a larger size had it been in normal health.

Fig. 14

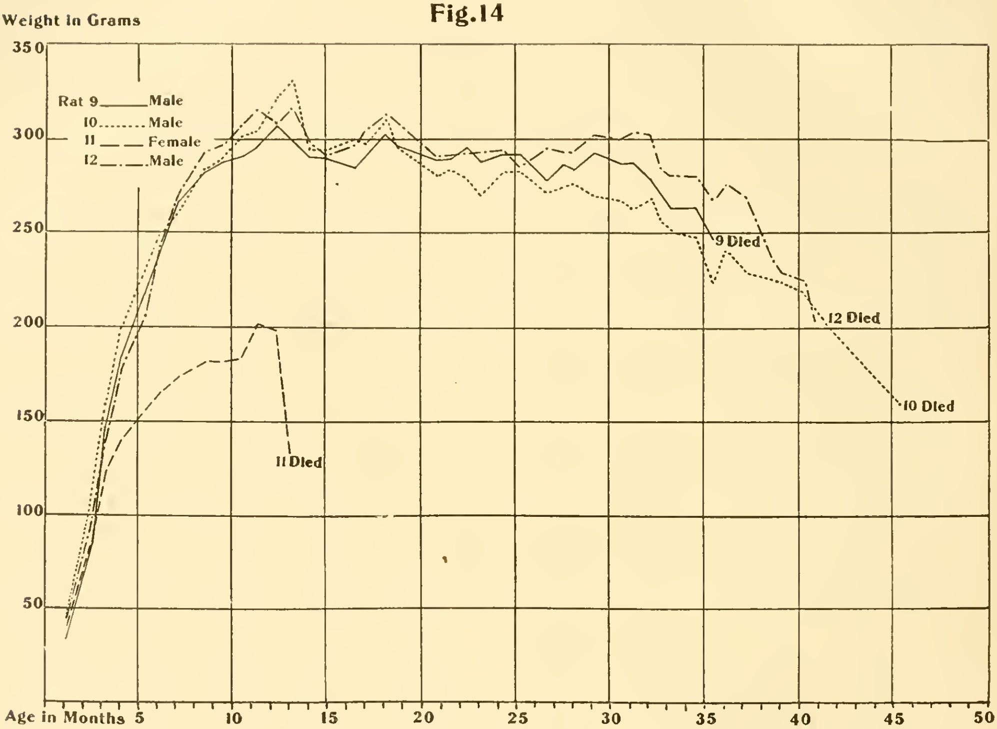

FIGURE 14. Curves representing the growth and decline of the control rats during their lifetime.

The curves representing growth show rhythmic fluctuations somewhat similar to those seen in the curves of daily activity in Fig. 11. These fluctuations in growth compare in a general way with those of daily activity. In some cases they compare directly in time and appearance. Only a few comparisons will be made. At 13 months old there was a general increase in daily activity (Fig. 11), also a general increase in weight

(Figs. 13 and 14), followed in each case by a noticeable decline in activity and weight. This decline was followed by another rise in activity and weight at about the age of 17 months. Again a similar rise though not so marked is noticed at the age of 23 months. These changes are often largely due to changes in food. Previous to and during each of these increases food richer in protein was fed than during the periods of decline. These changes in weight are not, however, always due to changes in food. Many other conditions enter to bring about these results, as they often occur when their diet is as nearly constant as possible. The following example will suffice. A very noticeable depression is seen ending the 21st month. During the two months previous to this date workmen were busy making repairs incident to the earthquake of April 18, 1906. These repairs necessitated a great deal of pounding and confusion in the adjacent room, which was connected by ventilators to the experiment room. Old plastering had to be removed and new put on, which caused a great deal of dust and noise. In this case I think it was the confusion and dust that caused the rapid decline in activity and weight as the diet had been such as to maintain uniformity in these respects.

I attribute the difference in weight between the control and exercised rats to the fact that the control rats did not have a ready means of exercise. They could give but a jump or two before they would be at the end of their run. In other words there was nothing to stimulate exercise. As a result the energy which would have been expended in exercise had favorable conditions been offered was used in growth. The experience of stock raisers corroborates this. Whenever they wish animals to increase in weight rapidly they pen them up. They have found that the same amount of food will fatten slowly or not at all when the animals are permitted a large range. This same idea is also advanced in poultry raising and production. It seems to me from the experiments on the albino rats that there are good grounds for such ideas. However, one must not lose sight of the product desired. If weight and rapid growth are desired feed the animal in close confinement. If agility and ability to perform work and endure fatigue are wished allow the animal considerable range which allows normal daily exercise.

TABLE V

SHOWING THE WEIGHT IN GRAMS, THE AGE AND THE AVERAGE WEIGHT AND AGE AT THE BEGINNING OF THE EXPERIMENT, THE PRIME OF LIFE AND THE DEATH OF EACH INDIVIDUAL

	Young						At Maximum Weight						At Death					
									Male Average		Female Average				Male Average		Female Average	
	No. Rat	Sex	Weight in Grams	Age in Days	Male Avg.	Female Avg.	Weight in Grams	Age in Days	Weight in Grs.	Age in Days	Weight in Grs.	Age in Days	Weight in Grams	Age in Days	Weight in Grs.	Age in Days	Weight in Grs.	Age in Days
Exercised Rats	1	M	42	30	41	37	252	544	262	534	176	509	214	746	212	819	128	1014
	2	M	43	30			272	497					206	925				
	3	F	37	30			176	509					128	1014				
	4	M	39	30			261	560					215	785				
Control Rats	9	M	34	30	41	39	310	392	320	391	200	337	*247	1074	241	1222	135	360
	10	M	45	30			331	392					165	1361				
	11	F	39	30			200	337					135	†360				
	12	M	45	30			319	390					172	1232				

* This weight was taken 14 days prior to death and is therefore heavier than it should be.

† Was killed because of sickness.

By consulting Table V and Figs. 13 and 14 it is readily seen that the greatest weight is attained at an earlier age by the control rats than by the exercised ones. The control males reached their greatest average weight of 320 grams at the average age of 391 days. It took the exercised male rats 534 days to reach their average maximum weight of 262 grams. The differences in weights and ages at death are also very pronounced.

Just how much work the control rats would have been able to perform at a given age and how it would have compared with that of the exercised rats at the same age can not be stated as it was not tested. But it is generally conceded that animals kept in close confinement, without a ready means of exercise, can not perform as much work and are much less agile than those having the chance of exercise.

So far as health is concerned it appeared that both control and exercised rats were in the best of physical condition. There was, however, a marked difference in the general behavior and attitude of the two groups. The control rats were slow in their movements, sluggish in appearance and seemed to lack energy, snap, and alertness. The exercised rats, on the other hand, were the reverse in all these respects. They were quick and very agile, bright in appearance, full of vim and very alert. Since these two groups of animals had the same food and practically the same environment it appears that the absence of a ready means of exercise on the one hand and the presence of it on the other is an important factor in causing this marked difference. Again the presence of exercise seems to be one of the causes for the difference in length of life of the two groups. Apparently the rats in the revolving cages not only used up some of the energy in exercise which would have gone into growth, but have also used up some of the energy which would have lengthened their lives.

A slight modification in the environment may probably have been instrumental in causing a difference in the length of life, possibly also of growth. The control rats were furnished with a large bunch of excelsior which they would fashion into a nest. The character of the nest depended upon the temperature. In cold weather the nest would be completely surrounded and covered over having a little tunnel for entrance, but in warm weather it would be open at the top. In this manner of living these rats had always an abundance of fresh air. The home of the exercised rat was a tin box with a certain number of ventilating holes through it and an entrance. It was furnished with sawdust and cotton for a nest. These rats were, therefore, more closely confined during their sleeping time than the control. The air was not so fresh and so free from odors. In fact it was hard to keep these boxes clean. These modifications in environment I think had probably something to do with the differences in growth and length of life in these two groups of animals.

CONCLUSIONS

From the foregoing experiments and data I think the following conclusions can be reached concerning the activity, growth and longevity of the albino rat.

1. The daily activity increases rapidly during about the first third of its life after which there is a gradual decrease until its death.
2. The change in amount of daily activity is not gradual but is rhythmic in character.
3. The character of activity during youth and old age is quite similar, being more or less distributed throughout the 24 hours. During the prime of life the activity is confined principally to night and rest to day time. In other words, the albino rat is nocturnal.
4. The female is much more active than the male.
5. Of the whole amount of work done during the life time almost three-fourths is done before reaching middle age. During the last 30 per cent of life only one-eighth of the total work was performed. During the first 30 per cent of lifespan almost three-eighths of the total work was done.
6. The males far surpass the females in weight.
7. The control males not only reach their maximum weight at an earlier age than the exercised males, but also greatly excel them.
8. The control rats lived longer than the exercised rats.
9. The exercised rats are more active, more alert and brighter in appearance than the control ones.

A NOTE ON THE RELATIVE VALUE OF PUNISHMENT AND REWARD AS MOTIVES

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Two figures

In his book on the dancing mouse, Yerkes[1] says, "The desire for food is unsatisfactory as a motive in animal behavior work, first, because a condition of utter hunger is unfavorable for the performance of complex acts; second, because it is impossible to control the strength of the motive, and finally, because it is an inhumane method of experimentation. In general, the method of punishment is more satisfactory than the method of reward, because it can be controlled to a greater extent." Since a large number of experiments in animal behavior have been made with the use of either punishment alone or with food alone as a motive, it seemed desirable to compare under uniform experimental conditions the relative efficiency of the two methods of training.

The tests described below were made on albino and black-and-white rats. All the rats were given the same problem, namely, to discriminate between two lights of different intensity. Some of the animals were *punished* for *incorrect* choices, and others *rewarded* for *correct* choices. The punishment was a light electric shock; the reward, milk-soaked bread. The rapidity of learning in the two cases was taken as an indication of the value of the method. It seemed desirable to test with still other animals the value of a combination of the two methods—*punishment* and *reward.*

The apparatus consisted of a discrimination-box (see Fig. 1), similar to the well-known box of Yerkes. The rat to be tested was placed in A, and the door I, leading into compartment B, was opened by the experimenter. After the animal had entered B, the door I was closed. The only way in which the animal could return to A was by passing through the compartments W or W′, thence through the corresponding doors V or V′ and into the alleys which open into A at O and O′. The door between the alley and compartment A was opened for the animal the moment it sought to enter A. The floors of W and W′ were wired to form a punishment grill, the two poles of

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[1] R. M. Yerkes. The Dancing Mouse. New York: The Macmillan Co., 1908.

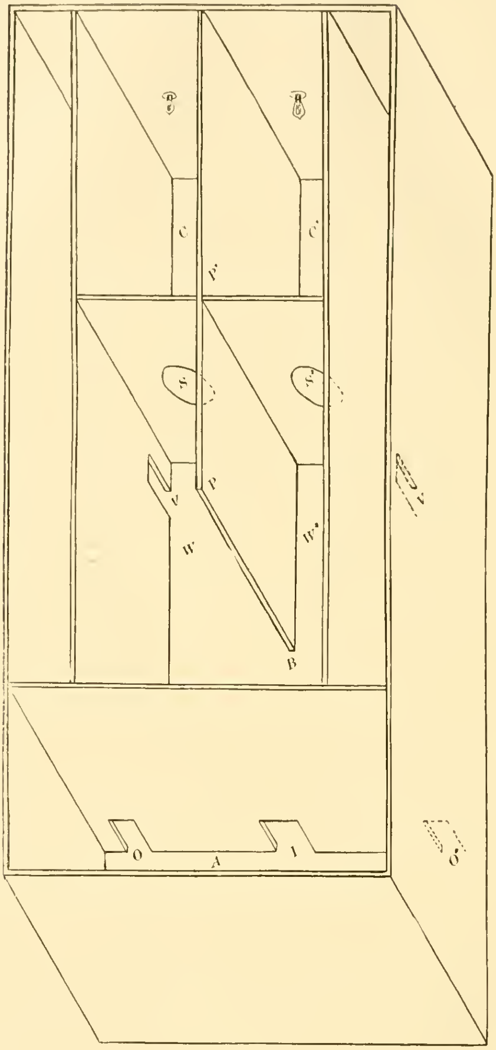

FIGURE 1. Discrimination box (after Professor Yerkes). A, nest-box; I, door leading into entrance chamber B. The floor of the compartments W and W′ are wired to give electric shock. P, partition between the two compartments; V, V′, doors leading into alleys which have doors O and O′ leading back into compartment. A, C and C′ are the light chambers, never entered by the rats. These are separated by the partition P′. The two chambers are light-tight and were kept tightly covered during test. S and S′ are the standard stimulus plates.

which were connected with a Porter inductorium. A convenient switch enabled the operator to cause the current to flow either through W or W′. Two Columbia dry cells No. 6 furnished the primary circuit. The position of the secondary coil was fixed at 7 cm. The stimulus lights consisted of two uncalibrated carbon lamps, 115 D. C., of 2 and 16 c. p. respectively, placed in the light compartments C and C′. The partition separating B from C contained two standard stimulus plates, S and S′, 5.5 cm. in diameter, placed 25 cm. between centers. The distance between the center of the plate and the floor of the box was 10 cm. Both plates were covered with flashed glass as recommended in the report of Yerkes and Watson.[1]

The tests were made upon light-adapted animals in a room the general illumination of which barely sufficed to show the movements of the animals.

Six female rats were used in the experiment. Five of them, Nos. 1, 3, 5, 7 and 9, were hybrid, and all from the same litter. Nos. 1 and 5 were albinos; 3, 7 and 9 were black-and-white rats, with pigmented eyes. No. 11 was a pure white rat from another litter. Preliminary experiments were begun upon the rats when they were 49 days old. All the rats were allowed to explore the experiment box for two days before preference tests were begun. In conducting the preference tests, 10 trials a day were given each rat. The experimental conditions of the preference tests were made the same for all the animals. At every trial food was placed in a small dish in each of the alleys V and V′. The lights were interchanged frequently and at irregular intervals, in order to exclude choice by position. The two lights occupied the two positions left and right an equal number of times. The two lights are designated, the one as b (brighter, 16 c. p.), the other as d (darker, 2 c. p.). Table I shows the record of the choices made by the animals.

From this table it will be seen that two rats had no preference; that one had a slight preference for the darker compartment, whereas three showed a preference for the brighter of the two.

In accordance with the generally accepted method of training against a preference, those rats which had been found in the preliminary tests to choose the brighter of the compartments,

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[1] Methods of studying vision in animals. Behavior Monographs, No. 2, 1911.

TABLE I

Rat	1	3	5	7[1]	9[1]	11
No. of trials	50	50	50	104	124	46
Choices of b	25	25	22	57	72	29
Choices of d	25	25	28	47	52	17
Preference	None	None	d	b	b	b

were made in the discrimination tests to choose the darker, and vice versa. In forming two of the groups the rats which showed no preference were grouped with those which had shown a preference. Three groups of two individuals each were formed. Each group of two contained one albino and one black-and-white rat with pigmented eyes. The following table shows the composition of the groups:

Group 1. *Punished* after choosing d and *rewarded* after choosing b. Contained—
Rat 1, Albino. No preference.
" 5, Pigmented eyes. Preference for d.
Group 2. *Punished* after choosing b. Contained—
Rat 3, Albino. No preference.
" 7, Pigmented eyes. Preference for b.
Group 3. *Rewarded* after choosing d. Contained—
Rat 9, Pigmented eyes. Preference for b.
" 11, Albino. " " b.

Group 2 was never rewarded. However, since the rats in this group showed great hesitation in entering B after having been punished a few times, they were occasionally fed in A, sometimes after a correct choice and sometimes after an incorrect choice.[2]

RESULTS

Table II shows the records made by the six rats. Each determination represents the percentage of error for a series of 10 trials. Ten trials a day were usually given. If, however, a rat made 10 correct choices during one day, he was given five more

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[1] Rats 7 and 9 were given a greater number of trials because it was found after the first 50 tests had been made that the two lights had not occupied the two positions an equal number of times. The extra trials were given in order to realize this condition.

[2] Several check tests were made in the course of all these experiments, which made it evident that the discrimination was effected by vision alone (by difference in intensity and possibly in color.)

TABLE 2

Percentages of Error for Daily Series of 10 Trials Each

No. of Ser.	Group I		Group II		Group III		No. of Ser.	Group I		Group II		Group III	
	Rat 1	Rat 5	Rat 3	Rat 7	Rat 9	Rat 11		Rat 1	Rat 5	Rat 3	Rat 7	Rat 9	Rat 11
1	30	50	50	50	40	50	32	10	20	30	20	30	30
2	40	50	30	60	30	50	33	10	20	40	40	30	40
3	50	50	40	50	40	40	34	0	30	30	20	50	40
4	40	40	50	50	50	70	35	10	10	50	20	40	50
5	20	40	60	60	30	30	36	20	20	30	20	30	30
6	50	40	40	60	60	50	37	0	10	10	0	10	50
7	40	60	70	60	40	50	38	10	10	80	10	40	50
8	40	50	40	80	50	60	39	50	20	30	10	50	20
9	50	50	60	50	30	40	40	0	20	20	20	30	50
10	60	30	20	40	60	40	41	10	10	10	30	20	40
11	50	40	60	50	60	30	42	40	30	20	10	40	40
12	30	20	40	30	50	30	43	0	60	20	40	40	30
13	40	40	30	80	40	40	44	0	10	60	10	30	30
14	40	40	30	50	40	40	45	20	10	20	20	30	40
15	30	20	30	30	50	40	46	20	10	10	10	30	40
16	20	20	30	50	40	30	47	0	20	20	10	40	30
17	30	50	30	30	40	50	48	0	0	20	20	20	30
18	30	30	10	30	30	40	49	0	20	20	20	20	40
19	30	30	50	10	60	20	50		0	20	0	40	30
20	30	50	30	20	20	50	51		0	20	0	30	10
21	40	30	20	40	30	30	52		20	10	30	20	40
22	20	30	20	20	30	30	53		0	0	10	20	30
23	30	50	30	40	30	40	54		0	0	10	20	20
24	50	40	60	20	40	20	55		0	0	20	30	30
25	20	20	70	20	30	10	56				30	20	50
26	30	40	30	50	30	50	57				20	30	20
27	20	30	40	20	30	40	58				30	30	30
28	40	40	30	20	50	30	59				40	10	20
29	20	10	50	20	50	30	60				20		
30	0	30	60	20	20	40	61				0		
31	0	40	60	20	40	50	62				10		

trials. When a perfect record for two days, i.e., 30 consecutive correct choices, had been made, the problem was regarded as learned. The table and curves show that the rats varied considerably in their daily record, as well as in the time required to learn the problem completely. Rat 1 (albino, group 1, *punished* and *rewarded*) completed the association in 490 trials. Rat 5 (pigmented eyes, of the same group), in 550 trials. Rat 3 (albino, group 2, *punished*) learned the problem in 550 trials, but rat 7 (pigmented eyes, group 2) had not learned the problem in 620 trials, at which time the work had to be discontinued. Neither of the rats in group 3 (*rewarded*) learned the problem in 590 trials, at which time work had to be discontinued with them also. The table shows also that the rats exhibited great instability even until the end of the experiment. A comparison of the errors of rats 3 and 7 shows that rat 3 had never made a wholly perfect series of records before her 520th trial, whereas rat 7 had made several perfect daily records before the 30 errorless choices were made in succession.

Fig. 2 shows the error curves for the three groups. The ordinates represent the percentage of error; the abscissae the number of groups of 10 trials each. It will be seen from these curves that group 1, which was both *punished* and *rewarded*, was the only group in which both rats made the discrimination. The curve of the *punished* group approaches that of group 1 more nearly than does the curve of the *rewarded* group, since one individual of the *punished* group learned the problem.

Several interesting observations were made during the work. The rats of the several groups maintained different attitudes toward the work. Those animals which received the electric shock showed after a few trials great hesitation before entering B, whereas the rats of group 3, which were never shocked, rushed into B without making any apparent choice. The rats which were punished became unwilling after a time to enter the electric box, and they assiduously avoided the door to this compartment, apparently associating the light with the shock. Great patience was often required of the experimenters, as several minutes would sometimes elapse before the rat could be persuaded to come to the door I. When at length it passed its head through this door, it would look to right and left several times before making a choice. This indecision would as often precede an incorrect choice as a correct one. However, as the discrimination became established, the animal ceased to fear to enter the electric compartment. It would come to the door I, glance only once to left and to right, and then dart toward the correct light. Although the rats which received punishment were at first unwilling to enter the electric compartment, they never became sulky as did the turtles used by Casteel.[1] The rats remained in good condition throughout

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[1] D. B. Casteel. The discriminative ability of the painted turtle. *Jour. of Animal Behavior,* 1911, vol. 1, no. 1.

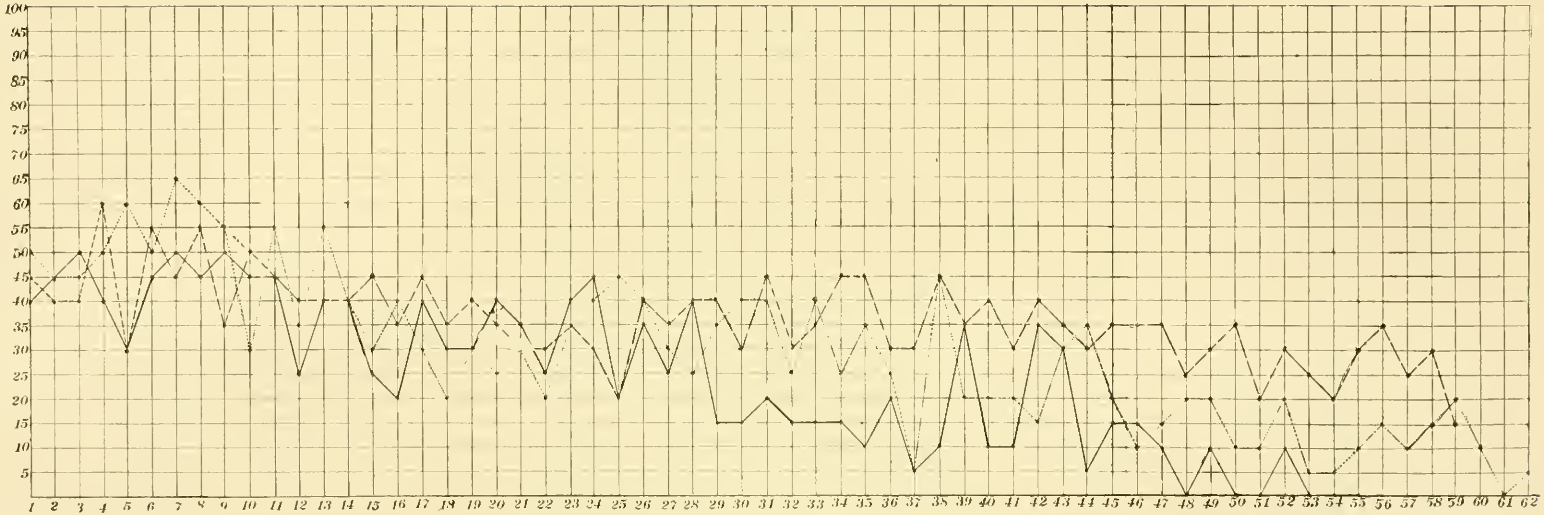

Figure 2. Error curves plotted from the data given in Table II. The numerals in the left margin indicate the percentage of error; those below the base line the number of the series, each series consisting of ten consecutive trials. The solid line (————) is the error curve of Group I (rewarded and punished); the dotted line(.........) is the error curve of Group II (punished); the dashed line (-------) is the error curve of Group III (rewarded).

the experiment. Except for the nervousness referred to above, none of the animals experienced any ill effect from the work, and no differences in the general health and activity of the three groups were observed.

The failure of the rewarded rats to learn the problem in the time allowed is of particular interest, since their preliminary work (preference tests) was along the same line as the work of the problem; whereas, in the case of the rats which were punished, a new element, the electric shock, was introduced.

The difference in the rate of visual discrimination between albinos and rats with pigmented eyes deserves notice. In each group the white rat made the discrimination before the rat with pigmented eyes. This is apparently not in harmony with the results obtained by Karl Waugh,[6] who states that "the discrimination of the albino mouse is not so good as that of the mouse with pigmented eyes." In neither the work of Dr. Waugh nor in the present work were a sufficient number of animals used to settle the question of the relative discriminative ability of the two varieties.

It seems evident from this experiment that a *combination* of *punishment* and *reward-motives* is more effective in bringing about visual discrimination in the rat than is either punishment or reward used alone. It seems evident, also, that punishment is more effective than reward, at least in so far as the rate of learning is concerned.

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[6] Karl Waugh. The rôle of vision in the mental life of the mouse. *Jour. Comp. Neurol. and Psychol.*, 1910, vol. 20, pp. 549-600.

BEHAVIOR AND COLOR CHANGES OF TREE FROGS

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INTRODUCTORY

Concerning the general facts of the life history, behavior, etc., of amphibia as a class much is well known. The admirable "Frog Book," by Miss Dickerson ('06) has made available a splendid body of information concerning "North American Toads and Frogs," and a similar service has been rendered by Boulenger ('98) in the "Tailless Bactrachians of Europe." But admirable as these are for the general student, they leave much to be desired in the way of detailed scientific investigation. This is particularly the case with phases of behavior, color changes, development, etc.

In the following account I have sought to bring together the results of observations and experiments touching facts of behavior and color changes in two species of tree frogs, *Hyla versicolor* and *H. arborea;* the former, the common tree frog of eastern North America, the latter the equally familiar tree frog of Europe. My study of *Hyla arborea* has been limited to about two months, and was done at Naples. In the case of *H. versicolor* my observations cover a period of several years, though at such incidental times as made it convenient and practicable. As will be seen, these studies include both field and laboratory work, especially of the American species, and I have made the field work emphatic wherever at all practicable. I have elsewhere ('09, p. 157) emphasized the crying need for larger attention to this phase of experimental work, believing that in many cases it is all but impossible to secure trustworthy results as to behavior of animals where the work has been done under such unusual, unnatural and artificial conditions as most laboratory provisions afford.

What right has one to assume that the actions of an animal taken rudely from its natural habitat and as rudely imprisoned in some improvised cage are in any scientific sense an expression of its normal behavior, either physical or psychical? Is it within the range of the calculus of probability that conclusions drawn from observations made upon an animal in the shallow confines of a finger-bowl, but whose habitat has been the open sea, are wholly trustworthy? It is no part of my purpose to discredit the laboratory or laboratory appliances as related to such investigations. They are indispensable. But at the same time let it be recognized that they are at best but artificial makeshifts whose values, unless checked up by constant appeal to nature, must be taken at something of discount. This must be especially the case with higher organisms. Some of these may, of course, be readily domesticated, or made more or less at home in aquaria or vivaria; but not a few absolutely fret their lives out, are never at ease, and probably never give expression to a natural reaction under such conditions. It seems to the writer that until one has been able to place his specimens under conditions approximating the natural, or has at least brought them to a state of semi-domestication, where in food-taking, evidence of health, etc., they are at ease, he has small right to dogmatize as to conclusions, or presume to make such conclusions the basis of so-called *laws of behavior*. Not a little of recent investigation along the lines of behavior has been vitiated at just this point, and must be repeated to be made trustworthy. The amazing mass of contradictory results which has loaded the literature of recent years is attributable to some extent to this misfortune.

In the following account the above precautions have been in constant mind, though owing to the peculiar habitats of these animals it has been attended with peculiar difficulties. In many cases it has been quite necessary to subject my specimens to laboratory conditions; but even then care was taken to simulate as far as practicable a natural environment. In certain cases this was done by enclosing a shrub by a fine meshed net, and thus leaving it almost exactly as if at home; and in all cases much attention was devoted to having specimens provided with leaves, moisture, etc., and with a supply of insect food, which afforded an approximation to natural surroundings. But even so, it is freely admitted that at best they are not wholly at ease; and behavior must not be too dogmatically interpreted.

BEHAVIOR

It may be assumed that in general tree frogs share about the same measure of intelligence as other members of the amphibian tribe. But even this would not place them high in that rank; for at the most favorable estimate amphibia are not credited with a large measure of mentality. But it may be doubted whether in general tree frogs may even claim so high a degree of intelligence, judged by their behavior, as many of their kin. It may be the peculiarity of habitat has induced a degree of stupidity in relation to danger from enemies, or possibly the color adaptations of certain species may have had something to do in this matter; but certain it is that they show less wariness or alertness than do the frogs whose habitat is upon the ground. In some other respects, however, they show a degree of cleverness which is not noticeable in the latter. For example, the facility with which they may leap and capture prey is quite remarkable. Again, the accuracy with which they seem to gauge distance is also striking. So that altogether it may be assumed that in rank of intelligence they are fairly comparable with their fellows of other genera and species.

The features of behavior which have had more particular attention are three, or perhaps four, namely: food-taking; light reactions; temperature reactions; hibernation. Since the last is largely a question of temperature it might perhaps be considered under that head. However, my observations on this feature are comparatively few and may as well be disposed of in this connection.

FOOD TAKING

In this Hyla behaves quite similarly to others of its kind. It seems not to notice any except moving objects. A spider may remain quietly in a given part of the cage for hours or days undisturbed. If it assume an active attitude it is almost certain to be taken very promptly. It is thus with any prey. The insects most commonly supplied were flies, small beetles, grasshoppers, spiders, etc. On one occasion a small wasp was released in the cage and at once began to buzz about or run up the sides of the cage actively. It was but a few moments ere a specimen leaped eagerly and captured the prey. Then a most interesting performance took place. No sooner was the wasp seized than it was whipped into the mouth, and in turn stung the frog; the frog in turn showed a very lively appreciation of that fact, and made an apparent effort to eject the creature; but the process of ingestion had gone too far, and deglutition was completed without further ado, nor did the frog show the least further sign of distress. On another day the operation was repeated and very much after the fashion of the preceding. It may be doubted whether amphibia show any particular discrimination based on that type of experience.

An interesting feature exhibited by Hyla was that of leaping to take its prey. Seldom does it show any disposition to stalk its prey, as do others of the class. It seems rather to wait till the prey is within leaping distance, which may mean several feet, and then with an easy spring it takes the victim with great accuracy, nor does it ever fail, so far I have seen, to safely lodge in a safe place. It seems not to detect prey at close range. I have seen, repeatedly, insects run over the body or legs of the frog, or indeed over its head and mouth, without a sign of recognition on the part of the frog. But let it be seen at some distance and the whole behavior changes at once.

Hylas are voracious feeders, and digestion seems to be fairly rapid though not very complete. It is quite easy to recognize in the feces the nature of the food taken. For example, a digested grasshopper is a *grasshopper still*! And so with a beetle or other insect. The chitinous exoskeleton seems only to have been softened and the internal proteid and other food elements to have been extracted and assimilated.

HIBERNATION

Presumably hibernation is a more or less similar phenomenon among all amphibia. In general its chief features are quite well understood. On this point my observations on species of Hyla add little that is new. Its first indication is expressed in a tendency to escape from unpleasant cold. Specimens in the laboratory show no disposition toward hibernation so long as conditions of temperature are conducive for activity. My specimens were in apparently normal activity during the months of November and December in the laboratory so long as conditions were favorable and food was supplied. The last week of December some five specimens were placed in a cool, damp corner of my cellar and observed from time to time. Very soon after this change they showed signs of dormancy, crept, or rather *backed*, for the mode of burrowing seems to be by means of the hind feet and the sharp posterior end of the body working together. In this way they burrow and bury themselves. My specimens burrowed backward under the moss and debris of the cage, crouched with head close to the bottom, closed the eyes and literally went to sleep. They were left for some time and in March I carefully inspected the box and found that a very small specimen had died. The others seemed normal. They appeared very sluggish, yet when turned over they righted themselves and resumed the sleeping attitude. Toward last of March they were again inspected, and to my great surprise and regret all were dead. Whether the hibernating conditions had proven unsatisfactory, or whether the inspection and handling to which they had earlier been subjected, was the occasion of the untoward ending one may only surmise. Perhaps something of both may have been involved.

Hibernation in a state of nature I have not studied. In one case I was fortunate in finding a specimen which was apparently just emerging from its winter sleep. It was found in the woods and very near where at the same time I found a specimen of the wood frog which had not yet emerged, but seemed in process of emergence, as the specimen was dug out from a bank of earth and leaves, and not yet sufficiently active to make any efforts to escape. In late autumn I have also found specimens on the ground along a wood path, and conditions of temperature were such as to suggest the presumption that they were even then seeking suitable hibernating quarters.

COLOR CHANGES

The general facts of color changes as a feature in the behavior of amphibia have been long known. Of course, tree frogs as affording some of the best illustrations of this phenomenon are no exception to this matter. In fact the European species, *H. arborea*, has been most critically studied of any, and has perhaps afforded the larger mass of existing knowledge on the subject. As will be seen in a later connection, some of the more striking aspects of behavior in color change have been studied in this species. So far as I am aware no one has made any of the

American species the subject of special study in this particular phase of behavior.

I shall begin my account with observations made upon *H. versicolor*, and later take up similar studies of *H. arborea*, with a comparison of the behavior of the two species as relates to color changes.

The present series of studies had their origin in some casual observations made upon *H. versicolor* in the early autumn of 1906, both as to color variations exhibited by specimens under essentially identical conditions of light, temperature, etc., and as to a peculiar habitat that seemed unique. Noting first the matter of habitat it may suffice to merely state the fact that the specimens were found perched upon the black iron fence of a cemetery along which was a cement walk leading to an open field often sought as a collecting ground for various specimens liable to afford laboratory material. The special position of the specimens was upon the flat surface of projecting supports for barbed wires strung above the fence proper. These supports like the other parts of the fence were painted dead black. Evidently they had climbed up the posts from the ground and had found a congenial resting position; but just what may have induced them to choose such a place it is impossible to suggest. But the fact of habitat aside for the time, my next point of curiosity was found in that the first specimen was very light grey in color, appearing almost white in contrast with the black support on which it was reposing. I merely paused to observe the little creature in its novel position, but came close to it to make the inspection exact, but without disturbing it in the least, as it seemed utterly unconcerned and unafraid, blinking sleepily, but without other sign of activity.

Somewhat further along the same course a second specimen was observed in exactly the same general position, perched at ease on the shelf, backed up against the post. In this case the color was very dark, and so in striking contrast to that of the first. As before, I paused for a few minutes and scrutinized the specimen as carefully as I could without actually picking it up, which might easily have been done, for it made not the slightest attempt to evade capture. However, it was passed with nothing beyond the cursory examination mentioned. A few days later I was passing along the same course with a friend, and about the same place found another specimen—possibly one of the same. We paused and I took occasion to point out some of the peculiarities already mentioned. Reflecting on the matter it occurred to me that perhaps some more painstaking observations, or possibly experiments might prove worth while. Accordingly on my next trip that way an observant eye was kept alert to note other specimens. Fortunately four were found, all occupying the same relative positions on different posts, but with the curious difference that no two were of the same color. One was very light, a whitish grey, one green, the other two very dark, one almost black. This was about ten o'clock, on October 10, and the day was bright and warm, all the specimens being in the sun. Following critical study of the specimens in position, they were then taken and carried back to the laboratory for further observations.

On a subsequent trip a few days later, several others were found in same location and always in the same positions and attitude. Of these one specimen was in shade, the others in open sunlight, and all were very light in color. On still later trips others were found, invariably in same general positions. It seemingly made no difference as to the hour of day, whether in sun or shade, whether the day were warm or cool; in almost every instance there was the same diversity of color markings and intensity. It was this seeming anomaly which first enlisted my attention and interest, though the peculiarity of habitat already alluded to was also involved in the interest.

EXPERIMENTS

As already mentioned, one of the first features of the subject to engage my attention was the seemingly extraordinary aspects of color difference characterizing the several specimens found under exactly the same environment. It is now my purpose to briefly describe the observations and experiments by which it was hoped some adequate explanation of the matter might be secured. Repeated field trips were made in order to find, if possible, some clue to the subject by a study of specimens under their natural surroundings.

On a previous page mention has been made of the records of two of these trips. A third trip secured three additional specimens; one was a very light whitish grey with the pigment figure sharply defined, a second was bright diffuse green, the pigment figure more obscure, and the third very dark, slaty brown, the figure pattern obscure. On a still later trip a single specimen was found, and was bright green. The light and position were the same as before. It was taken and carefully wrapped in soft damp paper and carried to the laboratory. When released in the experimental cage a half hour later it was found to have become a very dark, almost black color.

On a following season several trips to same locality resulted in finding some eight new specimens, all occupying the same positions as those of previous year. As before, there were marked differences in coloration. Of three specimens two were in open sunlight, and one in shade under large tree, and all very light whitish in color, with the pigment pattern sharply defined. Of two other specimens taken later one was almost black, the other almost white. The day was warm and bright in late September. In early spring, May, I found a single specimen which had apparently but recently emerged from hibernation. It was almost white, and presented marked contrast with the support upon which it was perched. These were taken to the laboratory as before, but in a closed collecting box and when released were found to be very dark. It should be noted that the last specimen mentioned was taken to my home and its subsequent behavior observed in a cage and in my garden lot.

From this account may be summarized the following: Of the fifteen specimens of which records were made, seven were whitish grey to pale grey; two were bright green; two very dark brownish; four slaty to blackish. This does not include several other specimens, probably about a dozen, but of these no permanent records were made, and so they are left out of account, except to state the general fact that they shared in general the same color variations exhibited by those more critically studied. It will be observed that we have here a range of color from almost white, through grey, green to yellowish, brown, slaty to almost black. Yet this range was to be observed in specimens occupying the same general positions, the same conditions of light and temperature, the same habitat. It seems rather obvious that the diversity cannot be explained by anything of an environmental nature. As will be seen in the later accounts of experiments similar diversity was evinced under identical tests.

But not only the anomalous color features call for explanation; there is also the hardly less anomalous peculiarity of habitat already referred to, which likewise is a matter of behavior the meaning of which is not at all obvious. I do not forget that occasionally these creatures may be found on old fences or walls. Still it is exceedingly rare to find them in such places. In all my observations, covering many years in a general way, only once have I actually found a specimen in such habitat, and then it was on an old rail fence not far from the wood. In this case it was in mid-summer, and the creature seemed quite at home in the devious labyrinths of light and shade, etc. This, however, is quite a different story from that here under review, and still leaves doubtful the why and wherefore.

Of course, the contiguity of the wood-side might suggest the possibility of descent from the trees looking toward hibernation. But when first observed it was in early September while the weather was still very warm, the trees in full foliage, and food quite abundant; too early apparently for the hibernating instinct to manifest itself. Nor could it be prompted by quest after food; for of all places this would seem the most unpromising. Even the elaborate webs of autumnal spiders were conspicuously absent on this fence. Nor on the other hand, could it have been prompted by unusual satiety of appetite, for when placed in the cages they were quite ready and eager to take an insect. I merely submit the facts without further comment, and leave the problem open to further light. It may be noted in passing that during one fall not a single specimen was found in this locality, even after diligent search. As hinted above, I am still disposed to regard the matter as in some way, not very clear, related to the approach of the hibernating season, the first expression of which on the part of the frogs would probably be descent from the trees. But even at best this is merely a guess.

In an account of color reaction in which an attempt is made to measure or determine with some approximation the amount of color change which takes place at a given time, or under a given test, it is very desirable that one have some basis of comparison. In the case of Hyla this is somewhat difficult. It has seemed, however, that the pigment patterns on the legs, and especially the dorsum, afford a fairly good basis. It seems quite as decisive as that of the lateral spines of the lizard, Phrynosoma, which Parker ('06) employed in his experiments. I regret not to have secured successful photographs of extremes in either direction; such figures are, however, admirably shown in "The Frog Book," Pl. XLVI.

As to the chemistry or mechanics of color change I shall not attempt a formal discussion or explanation. Those who are concerned with this feature may find extended details in the paper of Biedermann ('92), or in the paper of Parker (op. cit.). It may suffice here to merely say that skin color is due to the presence in the derma of certain pigment granules, usually contained in large pigment cells known as chromatophores, melanophores, etc. These granules are capable of migration distally, that is, toward the surface, or in the opposite direction, under certain stimuli. Certain other cells of the epidermis or subepidermis, known as guanin cells, form a reflecting layer which, on the contraction of the chromatophores act as a screen to obscure the pigment and at the same time reflect from their surface light falling upon it from outside. In a word, then, the distal migration of the chromatophores accentuates the appropriate color, while the migration in the opposite, or proximal direction decreases the color elements, and the guanin layer reflects the lighter color of its own cells.

In the laboratory the first attempt was to test the effects of light in relation to color. In the first series of tests five specimens were used, which were placed in glass jars of about two or three gallons capacity, perhaps nine inches in diameter by 15 inches high. Into one of these damp moss was placed, and in the other a branch with lateral twigs, and the tops closed by wire screens. These cages were at first kept in my private laboratory where a fairly uniform temperature was maintained, and where was a good diffused light from north windows. For experiments with direct sunlight the specimens were carried into an adjacent laboratory having east windows. The first experiments were simply the transfer of a cage from a remote and darker part of the room to a table close to the window, or even upon the ledge of the window. My records show a general lightening of the skin color as a result; but as will be seen later this inference was perhaps incorrectly attributable to the sole effect of light intensity.

To test the effects of direct sunlight the cages were taken from the diffuse light and placed on the table in direct sun. As a rule this had the effect of a rapid lightening of the skin, and the gradual fading of the pigment patterns of both legs and back. One of the specimens in this case rapidly assumed a green color in the light of the sun, while the other became whitish-grey. This experiment was made at nine o'clock and at two o'clock the green color persisted in the one, while the other specimen had become very dark, even darker than at the time of the test.

This experiment with direct sun was repeated many times in the course of the following few days, and usually with the same result, viz., the rapid lightening of coloration. It must be stated, however, that the green color was exceptional, and when it occurred was more persistent. In one case to be noticed later this green color became singularly permanent under a wide range of experiments, continuing over several days.

The next experiment consisted of putting one cage under a black hood, while the other was left as a control in the diffuse light of the room. After an hour the hood was removed but not the slightest change of color had taken place. Like the preceding experiment, this was repeated many times but with almost uniformly negative results. This seems in rather marked contrast with that associated with the capture of specimens and their transfer in the dark collecting box to the laboratory, in which the very opposite was usual, viz., the darkening of the skin. This also will be further considered in a later connection.

Two further experiments along this line are rather important. First may be mentioned the specimen taken in early spring, referred to in an earlier connection. This specimen when taken was of the usual whitish grey color. It was carried home from the woods and kept in an open jar on a back porch free from interruption. Experiments as to its light reaction seemed very prompt and more than usually positive. In a bright diffuse light it rapidly became light in color. Placed under a very dark hood for half an hour it had resumed the dark slaty color. The hood being removed a rapid lightening of the skin was very evident. The experiment was repeated several times and with similar results. However, when tried the following day the creature *failed utterly to respond to these tests.* On a still later day I transferred it to a jar into which had been placed a leafy branch and scattered green leaves. In apparent response the specimen gradually assumed a green color which spread over the entire dorsal portion of body and legs, and was most striking. A few hours later the leaves and branch were all removed, but the green color persisted. Repeated experiments of subjecting the specimen to darkness, placing it in open sunlight, change of temperature, etc., were of no avail; the color persisted for more than two weeks without essential variation, save in the matter of intensity. Finally, it was released in a tree in the garden lot and seemed perfectly at home, leaping to catch flies, ants, etc., but still no color change appeared.

A similar case appeared in the following fall. Three specimens were taken, one being green, one quite dark, the third very light. All were placed in an experimental cage and the following day a second had changed to beautiful green. The third day all were green, and so continued for five days. In the meantime another specimen had also been taken and was placed in a separate cage. When taken it was distinctly greenish, though not deeply so. During the process of transfer from field to laboratory it had become very dark, and continued so for an entire day. It then assumed a greenish hue which persisted for several hours, but later was replaced by the dark color. In neither of these had there been any proximity to leaves, or any green body. Details on this point will be submitted in a later connection.

Temperature. The intimate relations of temperature and light reactions have long been known; but only within recent times has this been made a critical study. Parker and Starratt ('04), and later Parker ('06), have shown rather conclusively the importance of both, but especially that of temperature in such behavior. One of the interesting conclusions of these observers is that light and higher temperature act in directly opposite ways, viz., that while light causes a distal migration of pigment and darkness a proximal one, heat on the other hand causes a proximal movement of pigment, and cold a distal one. It follows, on this assumption, that where these factors coöperate, i.e., light and low temperature, darkness and high temperature, we shall have the maximum expressions of the appropriate reaction. To quote directly Parker's statement, "Light causes a distal migration of the pigment granules of these cells; its absence a proximal one. A high temperature causes a proximal migration of pigment; a low temperature a distal one." He believes there are no real exceptions to these rules. I have not experimented with lizards, but from what has already been presented it will be seen that there are among tree-frogs very striking and positive exceptions, and accounts now to be cited will make this quite certain.

In my temperature experiments two chief methods were employed. First, and I think the more satisfactory, was the simple transfer of the cage from the room temperature to that of the outside air, which was easily done by raising the window and quietly lifting the jar from the table and placing it upon the outside window shelf; making accurate records of the temperature in each case. This feature could also be supplemented by utilizing the presence of a temperature oven which was within easy reach of table or window.

The second method, and this proved preferable at Naples, consisted in placing specimens in temperature baths, i. e., by means of ice or heat it was quite possible to have a vessel of water of any desired temperature, into which a specimen could be put and left the desired time without in any serious manner injuring it or interfering with its behavior.

Experiment. A jar containing two specimens was placed on the window shelf where the thermometer registered 14 C. At the end of one and one-half hours both specimens had changed from a dark brownish to very light grey. They were then placed on the inner window shelf where temperature was 21 C. and at the end of half an hour both had resumed the original color. In the room on the same table a control jar with three specimens had remained without change during the same time. The experiment was now reversed, that is, the jar was placed over a radiator shelf where the temperature gradually rose to 35 C. Both specimens showed signs of discomfort, moving about, shifting the body variously, scratching the nose with fore feet, etc., and in the meantime both had become markedly lighter in color, though not equally so. After a half hour the jar was transferred to the outer window shelf where in a half hour the temperature had fallen to 15 C. and both had grown almost white in color. Here we have one of the many examples of color change which negatives Parker's rules. The light throughout had been practically the same; hence the reaction must have been quite apart from any influence from that source.

Experiment. A jar containing three specimens was used, and the room temperature was found to be 21 C. At this the color of two was a grey over back, with distinct green on sides; that of the third distinctly darker and without trace of green. The jar was next placed on outer window shelf where the temperature was 4 C. One of the greenish specimens continued absolutely unchanged; a second became distinctly lighter and the green disappeared; the third remained in same position and color just as at beginning of experiment. After about an hour specimen No. 1 had become distinctly lighter, much like 2.

This experiment was repeated on a subsequent day. At the first, one specimen was very bright diffuse green, the other two were very dark colored. Room temperature about 21 C. The jar was placed on outer shelf where temperature was again 4 C. They remained here for more than half an hour and with only this color change, viz., the green specimen had become greener, the color having extended to the legs and become more intense. The color of the others had remained as at the beginning.

These experiments were variously repeated on these specimens, but in the same general manner, and with results so similar as to call for no special account. The following observations may be noted. On one day in early morning the temperature of the room had fallen quite lower than usual, viz., 12 C. Two frogs were found crouched under the moss and both very light, whitish grey; a third was crouched on top of the moss and very uniformly dark over body and legs. In the afternoon they were again observed, the temperature having become about normal, 20 degrees, and while all had continued in same positions, the two under the moss had become quite darkly colored, the other remaining as before.

Other temperature experiments will be described in connection with the account of *H. arborea*, and any special remarks and discussion may be deferred to a later time. Enough has been submitted, however, to confirm the suggestion made in an earlier part of this section that these frogs fail to sustain the theoretical rules.

Other experiments. In connection with the preceding observations and tests it was noticed in several instances that the mere handling of specimens often had the effect of involving color changes quite as marked as any of those already described. For example, in several instances it was noted that in carrying a cage of specimens from one room to another a perfectly evident color change was induced. To test this careful attention was given to color patterns of specimens in perfect quiet by two or three observers. Then a cage would be taken up and turned about, lifted up and then returned, etc., etc. Then another careful scrutiny would be made; and it soon became evident that almost any such disturbance might act as a stimulus. I even tried the effect of merely close scrutiny without any jostling or moving of the jars, and found that where a specimen could be brought to *observe* that it was observed, there was often a color change quite as evident as the others.

The following experiment will show that a mere stimulus is not of itself always sufficient to arouse a color reaction. I introduced the electrodes of a battery into a cage in such manner as to enable me to apply them to a specimen almost at pleasure. I was able thus to compel the specimen to leap vigorously about the cage for some time. But this was apparently without direct effect on coloration. This was the more surprising as other experiments of mechanical irritation with needle or forceps had *seemed* to act to induce a darkening of the skin. But similar experiments on *H. arborea* were frequently negative, hence it may be regarded as doubtful whether such stimuli serve to influence to any appreciable degree the coloration.

This does not, it seems to me, discredit the suggestion as to the fact of a nervous, or emotional factor as involved in these phases of behavior. So far to the contrary I am disposed to believe that we have here one of the important elements, possibly the *most directly* important, involved in this class of behavior. With this in mind one may seriously question whether the marked color changes almost always associated with the capture and carrying of specimens to the laboratory be not an expression of the *emotional* condition of the animal, rather than the mechanical effect of darkness, or other physical agent.

In this connection may be mentioned the case of one specimen which was taken in the wood and brought to my home. When taken it had been of the usual whitish grey coloration; but when taken from my box and placed in a large glass fruit jar it was very dark, almost black. Exposed to the light on the open piazza it very soon became very light colored. Covered with a black hood for half an hour it was found to have again become very dark. On exposure to light the whitish color reappeared. This was several times repeated and with similar results. Similar experiments the following day, and on subsequent days, failed utterly to show similar reactions. May one venture the suggestion that the creature had acquired such familiarity with conditions as to dissipate to a degree its emotional sensitiveness?

HYLA ARBOREA

During my recent occupancy of the Smithsonian Table at Naples, for the courtesy of which my acknowledgment is herewith tendered, I was fortunate in securing through the laboratory staff, some twenty specimens of this beautiful tree-frog, and was able to keep them under observation for an entire month. Normally this species is a bright green color over the back and sides and legs, while below it is pale pinkish to salmon color, with a black line extending from the eye along the side to the base of the leg. Considerable differences, or color variations are frequent and have been the occasion of several proposed species, as may be seen from any casual review of the systematic literature. (Cf. Boulenger, The Tailless Batrachians of Europe, p. 250.)

It need hardly be said that the color changes of this species has had considerable attention by many observers; but as is the case with the previous species, it has been for the most part rather general. So far as I am aware, the most critical and detailed investigations are those of Biedermann ('92). While very much in the results of this investigator must have an abiding value, especially those features dealing with the chemistry and mechanics of color phenomena, still there are others which have been questioned, and which I have not been able to confirm in the course of my studies. To certain of these I shall have occasion to refer repeatedly in what is to follow.

By way of introduction to this phase of my problem let it be said that, while many points of difference will be found as the two species are compared, I have been somewhat surprised in noting the many features of agreement which have come to light in their comparison. *Hyla arborea* shows the same individuality of behavior as does *H. versicolor;* it also exhibits the same erratic aspects of color reaction which have already been passed in review. From Biedermann's account I had confidently anticipated a degree of constancy in behavior which has not been realized under actual experiment. Let it be noted that my observations on this species have been restricted to the laboratory. I have not studied it in nature. Again, my work has been limited to a comparatively brief period, and to a single season. Except for the previous work with the former species I should feel less confidence in the results relating to the latter.

Two rather primary objects prompted my experiments on this species: First, a desire to compare its behavior with that of *H. versicolor;* and second, a review of the experiments and conclusions of Biedermann. As a preliminary to the latter it may be well to briefly summarize some of the more fundamental of his methods and results, but only in so far as they pertain to color changes. Furthermore, I shall not concern myself with the elaborate chemical details which occupy a large place in his investigations.

It has long been held, following the experiments of Lister ('58), that the nervous system was directly involved in these color changes, and further that they were under control of the animal, and operative only through the sense of sight. This view is held also by Boulenger ('97), who says, "That these rapid color changes harmonize, within certain limits, with the surroundings, is well known. The researches of Dutartre on *Rana esculenta* have shown that the changes are controlled by the animal, the contraction and expansion of the chromatophores not being due entirely to the direct action of light and moisture upon the skin, but also to a reflex action of the sympathetic nervous system brought about by the visual impression. Comparative experiments conducted on frogs possessed or deprived of the sense of sight, and placed in different conditions of light, show the blind individuals to change color much less rapidly." (p. 25).

Biedermann also admits the influence of the nervous system in relation to these changes, but holds it to be due to reflexes other than visual, contrary to the view of Leydig who holds with the foregoing to the direct influence of the visual reflex. According to Biedermann it is perfectly easy to bring about such changes of color by modification of conditions which affect contact stimuli. For example, according to his experiments, all that is essential in the case is that the surface be *smooth* or *rough*, or that conditions which involve such sensation be induced. This latter he held might be possible by section of the nerves of the foot, or by drying the foot and thus disturbing its normal sensory function. From experiments subsequently described it will be noted that these views have not been confirmed.

In my account of the many and varied experiments carried on during March and April at the Naples Zoological Station, I shall give in detail only those which have the most direct bearing on the special problem of color change. Naturally I was first concerned with those features which had been made prominent by Biedermann, as mentioned above. Among several kinds of cages or receptacles employed in the experiments, only two were found generally useful, viz., glass jars of some six liters capacity, and cages of wire gauze having a zinc bottom to afford means of supplying needed moisture, and a glass front to facilitate observation. Both were convenient alike for experiment and observation, but specimens seemed invariably more at home in the jars than in the cages, a matter of no small importance, and hence these were used for the most part.

Contact stimuli. As stated above, Biedermann has emphasized the importance of the nature of the stimuli acting upon the sensory nerve endings of the toes as determining factors in color changes in these creatures. It is in this way that he explains the reactions following the introduction of leaves into the vessel. " Wählt man nur solche aus und bringt sie (in dunkeln) in ein anderes Gefäss mit Pflanzenblättern zusammen, so ergrünt die Haut in kurzester Zeit. Der Versuch gelingt auch mit künstlichen Blättern und es beweist dieser Umstand allein schon, dass als auslösender Reiz wohl nur gewisse Berührungsempfindungen in Betracht kommen." (p. 506).

It will be noticed that this experiment was conducted in darkness, thus excluding the influence of either light or vision. By two other forms of experiment he claimed to demonstrate that by contact with a rough surface a darkening of the skin was produced, even to complete blackening. So simple seemed the experiments that no time was spared in putting them to the test. Accordingly I sought to duplicate the experiments, and placed the same number of frogs in the wire cage, the bottom of which was covered with a very coarse jute matting, and in the glass jar. After several hours under these conditions the only difference appreciable was that the color of most of those in the jar was definitely darker than those in the cage; just the reverse of what should have been the case. To vary the experiment a glass jar was prepared by enclosing a coarse wire netting over the sides, and coarse blotting paper over the bottom. Into this several specimens were placed and left for several hours, but all the while under critical observation. It was not possible to distinguish the slightest indication of color change. The jar was then placed under a black hood and again left for an hour. In one or two cases there seemed to be a slight darkening of the green, but it was not marked. The question of the influence of darkness will be considered in another section.

I next tried the experiment with leaves. Several of the darkest specimens were placed in a jar in which had been arranged a leafy branch and leaves scattered over the bottom. Two at once took up a position on the leaves, two clung to the sides of the jar, and one remained on the bottom. They were watched critically for two hours, but with only negative results, no change of color occurring. Next the experiment was tried of putting a jar of these specimens in a cool, dark room over night and early the following morning introducing into the jar a leafy branch and leaving in the dark for some hours. As before, the result was wholly negative. The experiment was varied by using the wire cage and a smooth glass jar each with the same number of specimens and leaving both in the same conditions of light and temperature during an entire forenoon. At the end it was found that in each case there were some specimens light green, some darker green, some very dark olivaceous. But it was not possible to distinguish that any significance attached to the nature of the cage or receptacle, or that conditions of contact could have played any rôle.

In the next place experiments were made to test the part played by the feet or toes as sensory agents. A specimen was placed in a very dry jar lined with white blotting paper, after having dried the animal very thoroughly with a dry towel. Another was similarly treated and placed in a jar lined with black blotting paper. These were put side by side on my table. It was not possible to detect any effect whatsoever in the coloration as a result after two hours' test.

One of the specimens then had the toe disks clipped off with sharp scissors and was placed by itself in a jar. This operation very effectually inhibited the climbing power of the specimen, but at no time was there the slightest evidence that it involved any modification of the conditions affecting color.

A still more heroic experiment was made along the same line, viz., the section of the spinal cord after etherizing the specimen. It was closely watched to see what effect the process of etherization might have; but neither in the anesthesia, nor yet in the actual section of the cord was it possible to detect any color response. Later the entire cord was destroyed, but still without effect on color. This experiment was repeated at another time, but in a reverse order, viz., first a section of the sciatic nerve, later section of the cord, and finally the decapitation of the frog; but as before with absolutely negative results. In this case bits of skin from different regions of the body were placed in a damp jar along with living specimens to note what effect this might have as to the color of such detached bits. It was not possible to detect any certain evidence of direct color change, though after some hours, there was a *deadening* of the color such as one nearly always observes on recently dead frogs.

It must appear, therefore, more or less evident that these results are somewhat in conflict with those referred to by Lister and Biedermann. They seem likewise in conflict with results reported by Rogers ('06, p. 168), to the effect that section of the optic nerves resulted in a marked darkening of the skin. He found, however, that a color variety of the same species failed to respond in this manner. Carlton ('03 p. 267), found also that section of the cord in Anolis had little if any effect on its color changes, which agrees in the main with my own results. He argues, however, " Since it seems impossible to explain the conditions without assuming some nerves to be involved, and since the only other nerves present are the sympathetic, these observations point to the sympathetic fibres as the ones controlling the change from green to brown."

It has been a growing conviction with the writer that experiments of this character as tests of behavior in particular, and also reactions of many other sorts, are extremely untrustworthy at best. The very nature of the experiment,—the anesthesia, the operation, and the consequent shock all conspire to involve the organism as a whole in such physiological state as to render reactions more or less abnormal, or even pathological. But in any case there must be the greatest care to have results confirmed by the most positive evidence from natural sources. And until we have larger confirmation from such experiments than is at present available prudence will suggest caution in drawing sweeping conclusions.

Darkness and *Light*. As shown in the case of H. versicolor, and as noted by other observers, the effect of bright light is often to induce a lightening of the skin color in amphibia. In some cases this takes place rapidly, even in 10 minutes under the strong influence of heat and light. This was found to be more or less true in the case of H. arborea, though the time involved in reaction to light is usually gradual. But it must not be overlooked that exposure to direct sun often involves a higher temperature. In several cases this was found to vary as much as 10 degrees C. within half an hour. From what will be shown in the next section it may appear quite probable that some of the changes one might be disposed to attribute to light are really induced by heat. But even here there are very conflicting results. However, the sum of numerous experiments go to deepen the impression that the usual effect of light is to induce a lighter skin color.

But on the other hand, my experiments of a converse character, viz., touching the effects of *darkness*, do not show any converse darkening of color. Many experiments were made to test this, and for the most part with negative results. In many cases the test was made by using a dark hood which could be easily slipped over the jar rendering the interior very dark. Of course, all degrees of time enable one to be certain that a given condition had not been overlooked or escaped attention. Another method, already referred to, was to place the jars or cage in a cool dark room for any desired time. Here one could inspect the results after an hour, or after any number of hours. The results from both methods were to the same effect, viz., that darkness of itself, at least in my experiments, has no appreciable effect as to color changes in this species.

One of the very significant features of all these experiments was the marked discrepancies in results. For example, in one case a specimen was transferred from a dark jar into direct sunlight. During the first 15 minutes no color change was apparent; the specimen became very restless, moving about constantly, and soon the color began to darken, and in 15 minutes more had become almost black. This is but one of several such cases.

Temperature. It has been concluded from many observations that temperature is often a feature of marked importance. Parker has so held in relation to Anolis and Phrynosoma (op. cit.), and others likewise have drawn similar conclusions. As shown in experiments with *H. versicolor*, so with *H. arborea*, this factor seems not to be of large value in relation to color changes. My experiments with the latter species on this point were made by two modes, viz., first by subjecting the specimen to room temperatures of different degrees; second, by subjecting specimens to baths of different degrees. Both these were used with this species, but the latter was by far the more convincing. By this mode water of measured temperature was prepared and a specimen placed in it for certain definite time. In the first of these temperature tests some five specimens were tried. Four of these showed very evident lightening of color when subjected to a temperature of 30 to 35 degrees C. for 10 minutes. But a fifth specimen grew positively darker under the test and at 36 C. had become almost black. To make it perfectly sure that these results were not due to the water alone others were tested in a warm oven of the same temperature, and with essentially the same results. This curious effect may be noted, that there was marked individual difference as to the effect of high temperature; a given specimen showing no distress at a temperature of 35 degrees or even 36 degrees C., while another would actually collapse at 30 degrees. This was repeatedly shown, both in the bath and in the oven. In every case, however, removal from the bath was followed by rapid recovery, except in one case where some two hours was necessary for complete recovery.

Following the tests for high temperature there was a subjection of specimens to greatly lower temperature. For example, a specimen was taken from the 35 degree bath and placed in one of 10 to 12 degrees. In this reversal of the temperature there was, as in the case of darkness, an almost invariable negative, i.e., there was no tendency to induce a corresponding darkening of the color. It frequently happened that the light color would persist for from one to several hours, though in most cases the resumption of the original color followed in about an hour. But in any case it was quite evident that the cold bath showed no tendency to hasten the matter.

It was said above that exceptions were not wanting. One has already been mentioned which showed a directly contrary color change. This was also the case with one other specimen. It remains to mention the fact that still others failed absolutely to show any response whatsoever. They were first subjected to a temperature of 30°, then to one of 35°, and finally to a still higher, but under the ordeal they remained neutral, or with a slight darkening of color in one.

Here as in preceding experiments with light we have not only such individual differences as might be expected, but we have actually contradictory results, or what *seem* to be such. The question may arise, Are they *really* such? In the light of all the facts I am prepared to answer this query in the negative. Not only are they not contradictory, on the other hand they but emphasize afresh the conclusion elsewhere enunciated by the writer that we are here dealing with sentient, volitional individuals, whose behavior is involved in exceedingly complex nervous and physiological states, the nature of which we may hardly dare to guess. That they are creatures subject to varying stimuli every day, and meet them variously, may be accepted as a matter of course. That in response to the more common of these stimuli there comes about a general correspondence or agreement in behavior by experience and heredity may likewise be accepted as a general conclusion not open to serious debate. But that *all must show the same reactions all the while and under all circumstances*, whether in their native habitat or under the artificial conditions of a laboratory, only he whose intellectual horizon is bounded by the bias of preconception and theory will be likely to claim. These conflicting (so-called) aspects of behavior are the exceptional methods by which a given individual endeavors to meet an exceptional condition. Colors in a living creature may be fixed, as in the dead pigments of feather or hair, or they may be the expression of the complicated play of living cells or tissues. In some cases they have become in the course of phylogeny more or less adaptive; and in some they seem under individual control. That they should at times exhibit differences and peculiarities need excite no startling exclamations of surprise or incredulity.

Other experiments. Various other stimuli were made use of in the course of my studies. Among them may be mentioned a few dealing with the matter of physical irritation, such as pricking with a needle, pinching with forceps, etc. These were tried with both species and in each series with essentially similar results. In such an experiment the specimen would be placed in a shallow box or dish, and by using a sharp pencil or sometimes a needle it would be made to leap actively about for some time, in some instances almost to the point of exhaustion. In the case of *H. arborea* the specimens would often emit during such treatment the most plaintive little squeaks at each leap, or when sharply pinched. But while in some cases these stimuli would be associated with a definite darkening of the skin color, at other times it would have just the opposite effect, the color becoming just as evidently lighter.

It was the same in case one pinched the skin of body or leg with the forceps. Some would rapidly darken, others become lighter, and still in other cases not the slightest change. So too, in holding with the fingers or forceps while the creature struggled to escape, some would change color, others would show no response. The experiment was also tried of picking up a jar with specimens and moving it quickly, or roughly about, or even shaking it from side to side; and here again with similar results. In these respects the behavior of *H. versicolor* and *H. arborea* show a very marked similarity, as in other points already referred to.

Still other experiments were made but they add nothing essential to those already described and need not cumber the pages further. Compared with the accounts given by others (vide supra), these may seem somewhat inconclusive; and but for those relating to *H. versicolor* they would be so to the writer. However, they are what they are,—facts to be reckoned with; and as such are submitted for the consideration of any who may be concerned with them. It may be worth while to merely mention the observation of Meyer, cited by Biedermann (op. cit., p. 489), to the effect that certain frogs (*H. temporaria*) are more responsive in reactions to light during fall and winter than in spring. "Wenigstens bleiben in dieser Jahrzeit die Thiere haufig im Licht dunkel, im Finstern hell." I cannot believe, however, this can be of importance so far as the general facts are concerned.

Psychology of Behavior. It is no part of my present purpose to enter upon a discussion of the intricacies of this feature of behavior, but merely to suggest a very few points in connection with certain aspects which have been submitted in earlier sections of the paper. Attention has already been directed to the fact that frogs must be regarded as having small claims to intelligence, and after one has had various species under observation for some time this verdict is not likely to be greatly modified. While it is true that frogs have succeeded about as well as the average of their class, and indeed better than most, if one may judge from the number of either species or individuals, still this has come about more by virtue of the peculiar instincts and habits than by the degree of intelligence which has been involved. However, one must not lose sight of such facts as are clearly indicative of intelligence. Both by observation and experiment it has been shown over and over again that frogs are capable of learning, they have memory, they profit by experience, etc. (cf. Yerkes, Instincts, Habits and Reactions of Frogs. *Harv. Psy. Stud.*, vol. I, 1903; Holmes, Biology of the Frog, Chap. XIX, 1907).

But, as already intimated, it is not my purpose to consider this aspect of the problem. I have been concerned in observing the very apparent association of emotional, and other neurological phenomena in connection with behavior. This was especially marked in certain cases of color change associated with handling, or close scrutiny. What was at first thought might be a direct reaction from darkness, as in the carrying of specimens in dark boxes, etc., was later found to be probably an emotional or nervous reaction due to handling. And the same was found to be the case in certain instances where specimens became aware of the fact that they were under observation. This happened too often to be regarded as merely a coincidence. And it seems altogether probable that some of the *apparent* conflicts of behavior are likewise due to psychic or emotional factors. Gadow cites cases in which male lizards while fighting exhibited marked color changes. And the facts herein described seem, at least in certain cases, to find their simplest explanations just along these lines.

In our zeal to interpret every aspect of behavior in terms of chemistry, or physics, impelled by a blind adherence to the law of parsimony, the tendency has been to proscribe any appeal to the psychical and to hold up to some measure of contempt any disregard of this rule or method. Far be it from me to invoke the mystical, or to read into animal activities a hasty anthropomorphism; yet as I have had occasion to point out in another connection, it is more than possible, indeed highly probable, that under the current stress of a *scientific fashion* not a little of current interpretation has been vitiated by the folly of a blind following of blind adherence to theory. He is a rash leader who would fix the metes and bounds of consciousness or mind in the world of the living. Surely it is no longer circumscribed by the genus Homo! If the first limit set at this point was by a procrustean *philosophy*, let us beware that the onus of a similar, not to say worse, blunder be not laid to the charge of a hasty *science*! There are other minds than ours, and they share something in common with us in those psychic powers which count for something in the stress of evolution, and as behavior in its manifold aspects gives expression to the endless struggle it is not strange to find involved therein the psychic along with other factors which go to constitute the organism and its environment.

SUMMARY

1. The species of tree frogs herein reviewed show on the average a lightening of skin color in the presence of strong light, but with marked exceptions.

2. Darkness has not been found to have any appreciable effect in relation to color changes.

3. A high temperature seems to act much as strong light to induce a proximal migration of chromatophores, hence to produce a lightening of skin color; but as in 1, with marked exceptions.

4. A low temperature has not been found to have any direct effect as to color changes.

5. It has not been found that the nature of contact stimuli has had any appreciable influence on color changes.

6. Color changes of these species of *Hyla* seems largely under nervous control. Various emotional and perhaps other psychical factors appear to be dominant in the behavior above considered.

As a final word I desire to express my obligations to the Director and Staff of the Naples Zoological Station for courtesies extended to me, and also to the Secretary of the Smithsonian Institution for the courtesy of an appointment to its table at this renowned laboratory.

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NOTES

A CASE OF INSTINCT

A young sparrow hawk fell out of its nest in the broken cornice of one of the college buildings and was brought to my office. I took it home and put it in a wire cage, set on a large stone at one side of the lawn, where it remained for two months. The first meal I gave it was of fish worms which I dropped into its mouth. The second day I fed it cooked liver cut into small bits, which I placed in its mouth. On the third day I placed a piece of roast beef half the size of an egg on the bottom of the cage. The hawk looked somewhat cautiously and then thrust out one foot and grabbed the meat, viciously sinking his claws into it. Its feathers ruffled, it spread its wings and fluttered about the cage still holding the piece of meat which it continued to strike with its bill, its other foot and its wings. With its outstretched wings, its curved back, its ruffled feathers and its savage thrusts with beak and claw, it seemed a much more formidable creature than it had when swinging on the swing a moment before. The transformation from the sleek, handsome, open-eyed bird to the infuriated beast was so sudden and complete as to be really startling. One could well imagine that such behavior would be effective in intimidating any small creature such as mouse or bird that might be so unfortunate as to fall its prey. Under the circumstances it seemed quite useless behavior in view of the already docile character of the roast beef.

I continued to feed the hawk on some form of cooked meat and the food always elicited the same sort of behavior. The extent of the demonstration seemed to depend somewhat on the hunger of the bird and the size of the piece of meat, great hunger and a large piece producing the more exaggerated behavior.

Since the hawk was evidently too young to fly when brought to me, it seems improbable that it had ever killed any animal or that it had ever witnessed such killing. If this is true we have here a clear case of the congenital tendency to kill prey. Whether one could analyze the hawk's behavior into that concerned directly with the act of killing and in addition certain elements of behavior which could only be useful in intimidating the prey I am not sure. I am inclined to think that such an analysis could be made. That the instinct is not absolutely rigid is evidenced by the fact that the stimulus lacked most of the characteristics that the normal stimulus would have. There was no movement, no feathers, no hair, no odor of living animal. It may have been the odor of the meat, for bread did not stimulate the behavior, nor did any other kind of food even though it was dark like the meat.

The hawk was quite tame from the start and I could handle it with impunity. Only once did I find out the razor-like quality of its beak and claws and that was when in changing it from one cage to another I hurt its wing. The dogs nosed about the edges of its cage but it was never frightened by them nor did it show any disposition to attack them.

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MODIFICATION OF THE INNATE BEHAVIOR OF COCKROACHES [1]

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Four figures

Graber's [2] experiments proved long ago that cockroaches, *Periplaneta orientalis* L, avoid the light and seek the dark. After having verified Graber's observations, I sought to change the direction of movement by teaching them, by means of the punishment method [3] to seek light and avoid darkness.

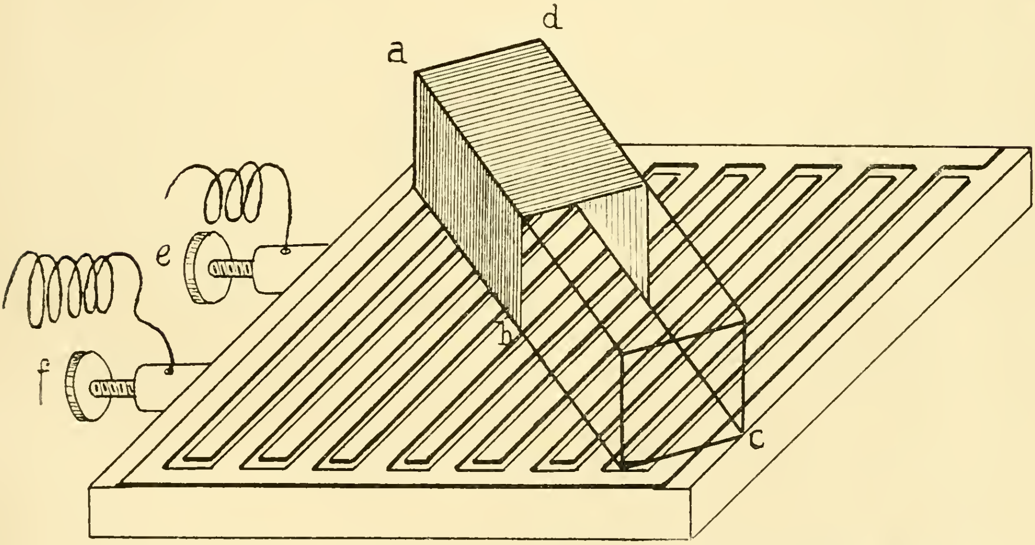

FIGURE 1. Apparatus for study of the modifiability of reaction to light in the cockroach. a c, glass-box; d b, covered part of box; e and f, electrodes.

The apparatus used in these experiments is shown in Figure 1.

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[1] The writer begs to acknowledge his indebtedness to Mr. Przibram, Director of the Institute for Experimental Biology in Vienna, for many helpful suggestions and criticisms.

[2] Graber, V. Grundlinien zur Erforschung des Helligskeits- und Farbensinnes des Tiere. Prag. 1884, S. 147–157.

[3] Yerkes, R. M. The dancing mouse. New York. 1907, pp. 98–99.

The base of the apparatus is formed of 29 insulated copper plates, each 0.5 cm. wide. The distance between plates is 0.1 cm. The unpaired plates are connected with a long plate on the one side of the base and the paired plates with *a similar* plate on the other side. The two long plates are connected with an induction apparatus and this, in its turn with an accumulator of 2 volts. By this manner of setting up the current, all the non-paired plates form one electrode and all the paired ones a second electrode. Above the base is a bottomless glass box, (Fig. 1 ac) 19 cm. long, 2.7 cm. wide and 8 5 cm. high. Two-fifths of this box is entirely covered with black paper (Fig. 1 db).

In order to determine at first to what degree cockroaches are influenced by the electric shock, I removed the glass box and placed in its stead another bottomless glass box, 16 cm. square, in order to prevent the animal from escaping. I then put a cockroach into this second glass box and observed that after running about for some time the animal came to rest. I then turned the electric current upon it. I began with a very light shock (distance of coils being 19 cm.) and increased the intensity gradually. The first visible reaction set in at an average of 14.5 cm. distance of the induction coils. This reaction showed itself by the animal suddenly moving about, running to and fro and trying to crawl up the glass walls. The intensity of the current at 12–13 cm. distance of the coils seemed to be most effective, and this intensity was used in all subsequent experiments. A higher intensity caused temporary paralysis, if used for a time.

All the animals I experimented upon were male larvae,[4] about one and a half years old, whose parents had been bred and kept in the Vienna Biological Institute.

During the definite experiments, and at the end of each series of experiments, the floor of the apparatus and the walls of the glass box were thoroughly cleansed with cotton wool moistened with alcohol in order to deodorize the apparatus.

After having replaced the covered glass box, Fig. 1, on the base, I started the definite experiments now to be described. A cockroach was placed in the lighter part of the box; it immediately turned towards the darker part, which it entered. Thereupon I turned the current upon it. The animal, on receiving the electric shock, ran wildly about, tried to crawl up the walls, and then ran back to the light part. As soon as it had retreated to the lighter region, I switched off the circuit. After a short time, the cockroach again entered the darkened part and received another shock. When this procedure had been repeated a variable number of times, according to individual differences, the animal running in the direction from light to dark, on attaining the boundary line between the light and the dark, would suddenly stop, move its head and thorax in all directions and begin rapidly working with its antennae. After a time it usually turned back to the light. This behavior is, I think, the first stage in the overcoming of phototropism by training; untrained cockroaches do not behave in this manner. After a short time the animal would again return to the darkness, and again receive a shock. Its behavior now completely changed, for it no longer ran about in the dark region, but at once retreated towards the light. Having attained the boundary line between light and darkness, it turned towards the light. The number of turnings back from the boundary line between darkness and light into the light without receiving a shock increased with training. I considered the training complete when the animal, being in the light, turned back to the light ten times on arriving at the boundary line between light and darkness, without receiving a shock. All of the ten cockroaches used in my experiments succeeded in doing this.

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[4] Mature animals differ externally from old larvae only by the presence of the pair of rudimentary wings.

During the experiments, when experience of the situation had already been gained, it repeatedly happened that an animal coming from the light to the boundary line between light and darkness, remained still of its own accord, with its head pointed towards the darkness. When I excited the animal to move by stroking its back (*dorsum*) by means of a strip of paper, it struggled against going into the darkness and turned towards the light in order to escape the irritating strip of paper. Table I shows the results of the experiments.

With the exception of Nos. 3 and 5, the cockroaches were tested at the same time of day, two o'clock in the afternoon, without intermission until they finally made the required number of turnings back. The time necessary for training was on the average from three-quarters to one hour. The training of Nos. 3 and 5 could not be finished on the same day because of the great fatigue from which they suffered. As the table shows, there are very great differences in their modes of learning.

TABLE I

Reactions of Cockroaches to Electric Shock when Moving Toward a Dark Box

Number of animal	Number of shocks	Number of turnings back
1	5	1
	12	5
	15	4
	16	5
	17	5
	18	1
	20	1
	21	5
	22	1
	25	3
	27	2
	31	3
	32	2
	33	1
	36	2
	37	10
2	6	1
	7	1
	23	1
	24	2
	28	10
3	11	1
	13	2
	21	1
	22	1
	41	1
	42	1
	52	1
	58	6
	64	5
	65	3
	67	2
	68	2
	69	1
	70	1
	71	1
	87	1
	(pause of 2 days)	
	106	3
	107	4
	112	1
	113	1
	114	7
	115	4
	116	10
4	7	2
	8	1
	9	1
	10	3
	11	1
	12	4
	13	8
	14	1
	15	7
	16	10
5	30	3
	32	1
	37	2
	38	2
	40	2
	43	4
	44	2
	45	6
	46	3
	61	2
	70	1
	(pause of 1 day)	
	87	1
	88	6
	94	5
	95	6
	96	10
6	12	1
	19	2
	23	2
	27	1
	35	3
	37	1
	41	8
	42	5
	43	1
	44	10
	19	3
	20	1

TABLE I—*Continued*

Number of animal	Number of shocks	Number of turnings back	Number of animal	Number of shocks	Number of turnings back
7	22	1		29	8
	25	2		37	10
	27	1			
	31	6	10	10	2
	32	10		12	1
				23	1
				37	1
8	6	4		39	3
	7	2		49	4
	12	1		50	2
	13	4		55	5
	14	4		56	1
	15	6		58	1
	17	8		63	1
	21	4		68	5
	22	2		82	1
	23	10		93	2
				94	1
				95	9
9	8	1		100	3
	12	3		105	1
	14	6		109	1
	22	6		112	6
	23	2		114	1
	28	4		118	10

While 16 shocks were sufficient for No. 4, No. 10 performed the ten obligatory turnings back only after having received 118 shocks. It may therefore be assumed from the different manners of learning that there were three types of cockroaches.

Let us mark the curves of learning based upon the results gained with the ten cockroaches. In all the curves, abscissas represent the number of shocks; and ordinates the number of the turnings back without receiving a new shock. Figure 2 shows the curves of Nos. 2, 4, 6, 7 and 8; Figure 3, those of Nos. 1 and 9; Figure 4, those of Nos. 3, 5 and 10.

The curves of the first series (Fig. 2) are similar in character, since they ascend successively till the crest is attained. The number of shocks necessary for producing the definite result is rather small. These data, however, prove that after the first experience of the shock, the animals made continuous progress. Thus the curves show no important or prolonged failure.

Fig. 4 represents the curve of the third series. This curve is much longer and shows that the animals learn only after having received many shocks. Perfection was gained only after a long time, but the fact that the curves Nos. 3 and 5 show two crests is particularly interesting. The one is a definite one, the other takes place about the middle of the course of the curves. The curve for No. 10 presents three crests. While the interest of the crests consists in their showing the progress of the learning, the space between the crests may be considered as the consequence of fatigue. I noticed external signs of fatigue, in all the cockroaches referred to, after the first crest had been attained. The animal remained motionless on the

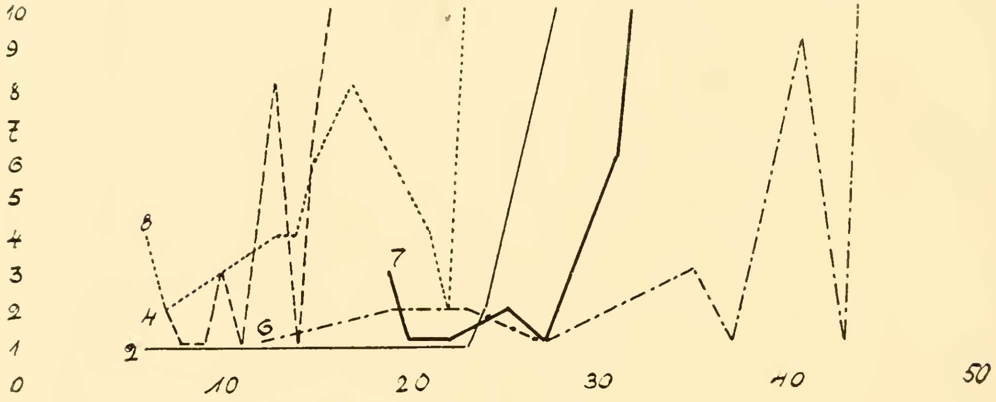

FIGURE 2. Representation of the reactions of cockroaches Nos. 2, 4, 6, 7, and 8. Number of turnings from dark box indicated on axis of ordinates. Number of electric shocks indicated on axis of abscissas.

same spot or when excited to movement shot blindly along the walls, failing to stop on the boundary line between light and darkness. In the same way, the movements of the thorax and the antennae, previously described, ceased when the animal had arrived at the boundary line. The effect of the electric shocks upon the animal seemed to be lessened. As soon as the animals had rested, they were able to attain the second crest and the experiments were completed.

The tests of No. 10 were achieved in the same trial, those of No. 3 after the elapse of one day, and those of No. 5 after two days. Thus the two elements of these curves, crests and failures, refer to two factors concerned in the process of learning: *practice* and *fatigue*.

The curves of the second series (Fig. 3) resemble those just described in that they also show two crests. The difference consists in the small number of shocks necessary to obtain definite results of learning. This circumstance, however, brings it near to the curves of the first series. Thus the curves of the second series take an intermediate position between the two others.

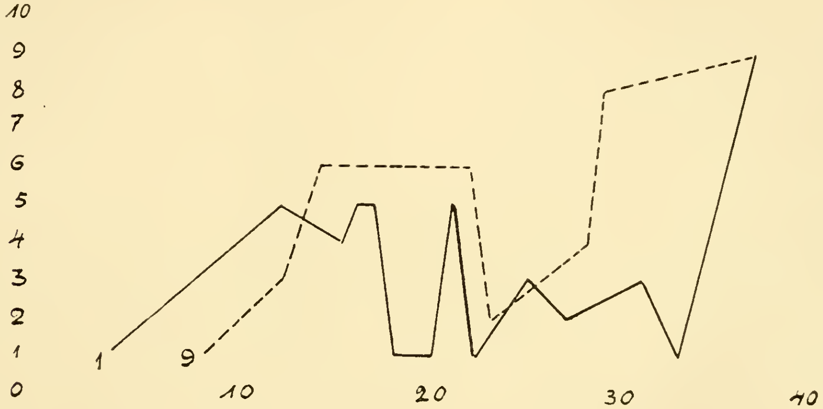

FIGURE 3. Representation of the reactions of cockroaches Nos. 1 and 9.

The results of an analysis of the curves enables us to separate the cockroaches tested with respect to their ability in learning, into three groups as shown in the following table.

	Characteristics of the curve	*Characteristic features of the cockroaches in learning*
1	Short	Rapid progress with practice
	One crest	Slow fatigue
2	Short	Rapid progress with practice
	Two crests	Rapid fatigue
3	Very long	Slow progress with practice
	Two crests	Rapid fatigue

The analysis of the curves and the possibility of isolating two factors, *practice* and *fatigue*, permit us to assert that the learning-curves of cockroaches may be considered a special case of Kraepelin's "Arbeitscurve." [5]

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[5] Kraepelin. Die Arbeitscurve. Phil. Studien, 1902, Bd. 19. S. 459–507.

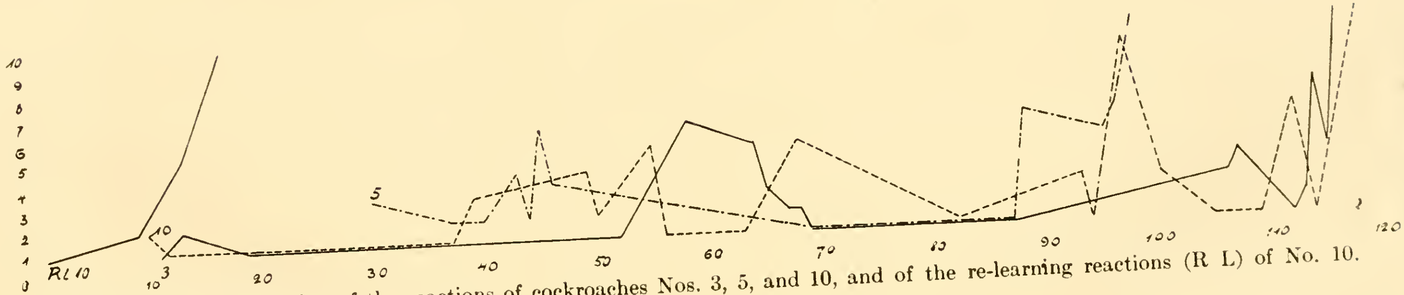

FIGURE 4. Representation of the reactions of cockroaches Nos. 3, 5, and 10, and of the re-learning reactions (R L) of No. 10.

Marked individual differences were noted with respect to the time during which the cockroaches retained their newly acquired habit.

The data given below were gathered by measuring the time elapsing between the last effective shock given and the animal's retreat into the dark part of the glass box.

TABLE II

No. of animal	Time of permanent avoiding of the dark
3	3′ 53″
9	10′ 57″
4	11′ 20″
8	11′ 48″
5	13′ 3″
2	14′ 24″
7	18′ 32″
10	25′ 15″
6	55′ 0″

No relation is evident between the degree of permanency of the newly acquired habit and the number of shocks necessary to establish it. Though the training influences the behavior of the cockroaches only for a short time, the memory of it lies latent, as is proved by the rapidity with which they relearn. An animal once trained after a few days performs the obligatory number of turnings-back after only a few shocks have been applied. The following table shows the results of relearning.

TABLE III

No. of animal	Number of shocks necessary for learning	Number of days between learning and relearning	Number of shocks necessary for relearning
10	118	1	17
9	37	2	12
8	23	3	4
4	16	6	5
1	37	9	7

The curves of relearning resemble those of the first series. They indicate rapid progress without noteworthy fatigue. As an example of the curve of relearning, I give that for No. 10, Fig. 4, R L, 10.

Before concluding my description of the experiments, I must mention that it was possible to establish the habit in an animal whose antennae had previously been removed. A hundred and twenty-six shocks were required before the animal performed the obligatory ten reversals. The learning-curve is the same as those of the third series in normal animals.

Felix Plateau [6] has recently stated, as a general conclusion drawn from his experiments with bumblebees, that "chez Bourdous et vrais-semblablement chez les autres insectes la mémoire des faits n'existe pas" (p. 159). The results of my experiments do not support this statement.

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[6] Plateau, F. Les insectes ont-ils la mémoire des faits? L'année psychologique 1909, t. 15, pp. 148–159.

THE REACTIONS OF THE FLAGELLATE *PERANEMA*

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One figure

Peranema is a colorless, cigar shaped flagellate scarcely 0.05 mm. long and but little more than 0.01 mm. in diameter. A prominent contractile vacuole is found near the anterior end from which projects a heavy flagellum nearly as long as the body, Fig. 1. It is a hardy, sluggish creature, usually found in abundance in cultures rich in decaying organic matter. I have kept specimens in excellent condition for three weeks on a slide under a cover glass sealed air-tight with vaseline. These flagellates ordinarily move in contact with solid objects or on the surface film, but sometimes they swim through the water very much like Euglena. I shall refer to the former method of locomotion as crawling. The rate of motion by either method is relatively slow, the crawling rate being only 1.3–2.6 mm. per minute and the swimming rate but very little more. Owing to this very moderate rate of locomotion it is a simple matter to follow under high magnification every movement the animal makes.

In crawling, Peranema is frequently seen to continue for considerable distances in a fairly straight line. It does not rotate as it does in swimming but proceeds with a given surface continuously in contact with the substratum to which it adheres with considerable tenacity, probably by means of secreted mucus. The flagellum extends straight forward and is rigid and motionless except for about one-sixth of the whole at the tip, which is bent at right angle so as to form a sort of hook, Fig. 1. This bent part of the flagellum vibrates rapidly, striking backward and forward in such a way that the free end describes a narrow ellipse. Thus the creature is literally drawn slowly and steadily along. This is the method of locomotion that is ordinarily seen but if the animals are strongly stimulated, as by the addition of a little iodin, marked wave-like contractions are seen to pass over the body from the posterior to the anterior end. That this wave-like contraction is at least at times functional in locomotion is shown by the fact that specimens with the flagellum removed, an operation which was repeatedly performed with a fine glass rod, are still capable of progressing, although much more slowly than before the operation. The activity of the flagellum is, however, alone sufficient to produce locomotion, for specimens are frequently seen moving quite actively and accurately in the direction of the flagellum when the body is bent at right angles to it, so that contractions in the body could not possibly function and still admit of movement in the direction of the flagellum. When iodin is added it not only induces marked contractions in the body but also much more vigorous action of the flagellum. Not only do the rate and the extent of vibration increase but a larger portion becomes active. Under such conditions the portion of the flagellum which vibrates increases from about one-sixth to more than one-half, the angle in it being much nearer the body. The combined effect of the increased activity in the flagellum and the contractions in the body causes the rate of locomotion to become almost double that ordinarily observed in crawling specimens.

If the iodin solution is strong enough the crawling specimens leave the substratum and swim freely through the water. I have never seen this method of locomotion under ordinary conditions; it must be very rare in nature. In swimming, the entire flagellum is active, having a motion somewhat like that of a snake except that it is not restricted to one plane but rotates continuously on its long axis so that it appears much like a cork-screw in motion. The body also rotates continuously when the animal swims, usually counter clockwise, like most of the free-swimming unicellular organisms, but the rotation of the flagellum is independent of this; it may often be seen when the body is stationary. As a matter of fact it is the rotation of the flagellum that causes the rotation of the body.

Reactions to Contact Stimuli.—The study of the reactions of Peranema was confined almost entirely to crawling specimens. As these animals move about they come in contact with objects, the tip of the flagellum usually striking the object first. Often when this happens there is no response. This is especially true when the object is small or when it consists of a mass of loosely compact granules or fibers, or when the flagellum strikes one side of the object. Under such conditions these creatures usually slide by or through the obstruction, their course being altered only in so far as the mechanical resistance requires. Ordinarily, however, there is a definite response when the flagellum touches an object. The organism stops, the body bends sharply always toward the larger lip, throwing the anterior end with the flagellum, usually more or less curved and inactive, to one side

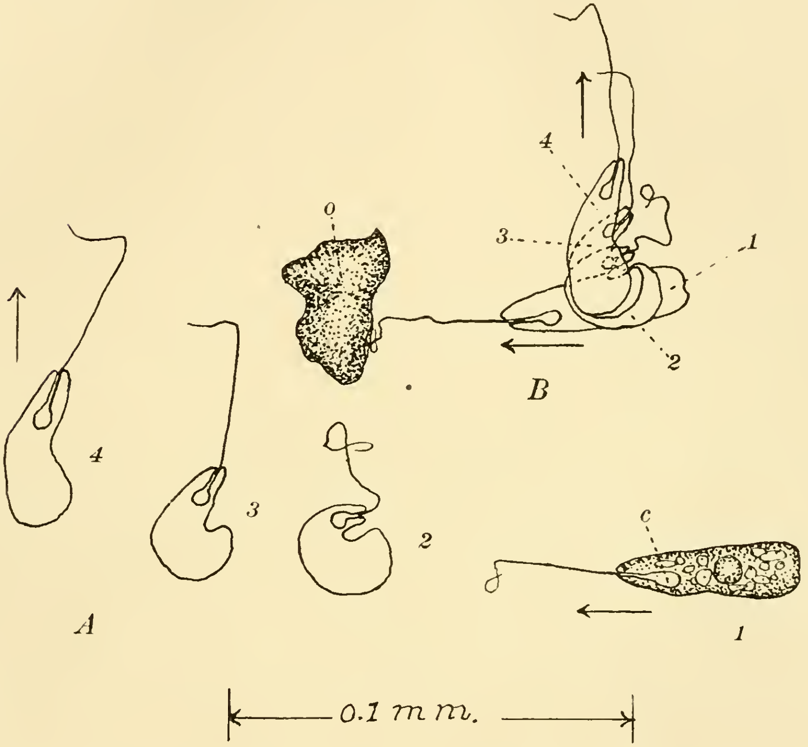

FIGURE 1. Camera drawings of specimens of Peranema representing the reaction to contact or chemical stimulation. A, Outline of four individuals selected from many suddenly killed in Worcester's fluid in different stages of the response. B, Outline of the same individuals superimposed to represent the relative positions of a specimen in different stages of the response; 0, solid object; 1, specimen crawling in the direction of the arrow, only the bent tip of the flagellum is active; c, contractile vacuole; 2, position taken immediately after stimulation; the flagellum is inactive, the turning of the anterior end being due to contraction on one side of the body; 3 and 4, later positions assumed, the body gradually straightens and the tip of the flagellum again becomes active. Note that the response results in a change in the direction of motion of approximately ninety degrees.

through an angle of nearly 180 degrees, Fig. 1. Then the body gradually straightens again, the anterior end turning back somewhat toward its former position, the flagellum straightens, the tip becomes active and the creature proceeds on its new course, having changed its direction of locomotion approximately 90 degrees. In this way objects are avoided which frequently have been only slightly touched by the very tip of the flagellum. This bending reaction is not dependent upon the activity of the flagellum as maintained by Verworn (Allgemeine Physiologie, 1909, p. 451). It is entirely due to contraction of one side of the body. Moreover, I was unable to obtain any evidence indicating that the direction of motion in crawling specimens is changed in any other way. Neither the beating of the flagellum nor any thing connected with the wave-like contractions of the body functions in changing the course of this animal.

The fact that Peranema responds when only the tip of the flagellum is touched indicates that this structure is sensitive to contact. It does not, however, prove that it is, for it may be that the response is due to change in pressure on the anterior end of the body owing to the contact of the flagellum or to mechanical waves transmitted through it to the body. However this may be, the animal responds precisely the same when the body is touched with a glass rod, as was repeatedly done. Moreover, this response bears no relation to the location of the stimulus on the body; it is precisely the same no matter whether the posterior or the anterior end is touched, or any other point on the surface. There is no indication whatever of a differential response to localised stimulation. Peranema differs in this respect from most of the other unicellular forms that have been tested. When Lacrymaria or Stentor e. g., is stimulated on the posterior end it ordinarily does not respond the same as when it is stimulated on the anterior end. Thus the character of the response in these creatures depends in some measure upon the location of the stimulus, while in Peranema this is not true. But in this animal there may be local response to local stimulation. One often sees contraction of the tissue in the immediate region to which the stimulus is applied, and these contractions may spread over the body in wave-like form. Whether or not the direction and the character of the wave is dependent upon the location of the stimulus I am unable to say. At any rate the direction of motion is not changed by reactions of this nature, although the rate may be.

Thus we see that the reactions of Peranema which regulate the direction of motion are in the nature of trial movements. There is no indication of orientation and procedure directly toward or from objects or solutions, nothing in the nature of a tropism as defined by Loeb. When the animal is stimulated, no matter how or where, it either moves more rapidly or stops, bends toward a given side and takes another course. If this relieves the stimulus it continues, if not it repeats the reaction until the stimulus does not again occur.

In this connection it is interesting to consider the reactions of specimens entangled in debris. The first response seen after a specimen gets into a fibrous mass which affords some obstruction is an increase in the activity of the flagellum. The tip does not only strike back and forth more vigorously but a larger portion becomes active. If this does not result in freeing the creature it responds by bending the body sharply, i.e., with the bending reaction, and starts in a different direction, after which, if still held fast, it may again lash the flagellum furiously and then again change its direction by bending the body. Thus it continues alternately trying the two different reactions until it becomes free. This indicates that we have here an adaptive change in the character of the response without any change in the environment such as Jennings discovered in Stentor; for it is hardly probable that these different responses can be accounted for by changes in the pressure of the tangle owing to variation in the activity of the flagellum or to the bending of the body.

Reactions to Chemicals.—My object in studying the responses of Peranema to chemicals was to ascertain if possible whether or not the flagellum is sensitive. In these observations colored substances were introduced under the cover-glass either in solution by means of a fine capillary pipet or as crystals by means of a fine glass rod. The reactions of the animals were then studied as they swam toward the colored substance which gradually spread out in the process of diffusion.

The responses to chemical stimuli consisted in all cases observed of sharp bending of the body and an abrupt change in the direction of motion just as in the reaction to mechanical stimuli.

However, in the case of iodin-green crystals introduced dry the animals ordinarily reacted as soon as the tip touched the colored solution; while in the case of methyl-green introduced in the same way they usually did not react until the body was entirely or nearly surrounded by the colored diffusion from the crystals; and in the case of methyl-blue introduced dry or chromic acid or iodin in solution they responded before the tip of the flagellum reached the colored diffusion. In these reactions there was no indication whatever of orientation in the direction of the lines of diffusion as demanded by Loeb's tropism theory. The animals in all cases crawled about in all directions and came in contact with the solution at all angles. Our evidence, however, leaves us in the dark as to the sensitiveness of the flagellum, for the creatures responded in one case before the tip of the flagellum reached the colored portion of the solution, showing clearly that chemical diffusion so weak that the color is invisible may be still strong enough to stimulate the animals. Thus the possibility of direct action on the body itself of chemical solutions too dilute to be visible was not excluded in any case.

Reactions to Light.—Specimens of Peranema under the microscope were at different times suddenly exposed to light varying in intensity from zero to direct sunlight and then suddenly shaded again. In the case of the lower intensities there was no indication of any response, but in direct sunlight it appeared at times as though the flash of illumination induced the typical bending reaction, especially in specimens which had been kept in light of low intensity for some days preceding the experiments. But at best these responses were so indefinite that if there really are any to light they can be of very little significance under natural environmental conditions. There is no evidence whatever of orientation in horizontal rays of light such as is found in Euglena and other similar organisms.

The several different methods of response observed in Peranema may be described somewhat as follows: (1) Increase in rate of locomotion; (2) Change from crawling to swimming and *vice versa;* (3) The becoming active of a greater portion of the flagellum than ordinarily takes part in the activity; (4) Augmentation in the wave-like bodily contractions; (5) Local response to local stimulation; (6) Bending of the body toward the larger lip.

SUMMARY

1. Peranema usually moves in contact with the substratum by means of a combination of wave-like contractions of the body and the flagellum, but it can move by the action of either alone. The action of the flagellum is, however, ordinarily much more effective in locomotion than the wave-like contractions.

2. When very strongly stimulated, as by the addition of chemicals, it swims through the water free from the substratum much like Euglena. This is rarely seen under normal environmental conditions.

3. When crawling the flagellum projects straight forward; only the tip is active, and the organism usually moves on a straight course without rotation on the long axis. When swimming the entire flagellum beats in a rotary wave-like fashion, and the body rotates on its long axis and takes a spiral course.

4. Peranema does not orient. It responds to light very indefinitely if at all.

5. If the flagellum comes in contact with an object (tactile stimulation) the animal bends always toward the larger lip, then proceeds on a new course more or less nearly at right angles with the old. This same response can be induced by contact stimulation of any part of the body or by chemical stimulation.

6. There is sometimes local contraction at the point of stimulation but this does not appreciably influence the direction of motion.

7. If the body becomes lodged or entangled in debris while the animal is crawling, the tip of the flagellum gradually beats more and more vigorously and a larger part becomes active. If this does not free the animal it bends toward the larger lip so as to extend the flagellum in a different direction. This then beats again as before, after which the body may bend again. Thus the bending of the body and the activity alternate until the animal becomes free or fatigued. This behavior seems to indicate that we have here different methods of response to the same stimulus.

HUMAN REACTIONS IN A MAZE

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From the Psychological Laboratory of the University of Chicago

Seven figures

The purpose of this experiment is to compare the ability of human adults and children to learn a maze with that of white rats under approximately comparable conditions. We shall attempt to correlate the differences in the objective records with the differences between the three groups in intelligent ability with the hope that such a study will throw light upon the general problem of the interpretation of a learning curve in terms of intelligence.

The serious difficulty involved in the experiment is the attainment of conditions which may be regarded as in any degree comparable. The human maze was constructed out of doors in a small lot surrounded by a high board fence. This fence eliminated extraneous sources of disturbance and secured a privacy essential to good work. The plan of the maze is represented by Fig. 1. Its dimensions are 14 x 20 feet, with alleys of 2 feet in width. The true path is 78 feet in length, complicated by nine *cul de sacs*. The runways were constructed by stringing on posts strands of wire 2½ feet from the ground. The ground was leveled and covered with gravel so as to render the paths as homogeneous as possible. The subject is compelled to run the maze blindfolded and he is guided by contact of the arms or hips upon the wire strands constituting the sides of the runways. The construction of a maze of this size with high wooden sides was deemed impracticable. It is doubtful if the sides of the alleys of such a maze could be made as homogeneous to the human eye as they are to the rat. However, the desideratum in the experiment is the attainment of comparable subjective conditions rather than comparable objective conditions. Previous work has indicated that our mazes are so homogeneous that visual distinctions play an insignificant rôle in the rat's behavior. The rat learns the maze apparently in kinaesthetic and contact terms. Our purpose was to study the ability of humans to learn a maze under similar conditions. The elimination of vision was thus essential to the attainment of our purpose with any degree of probability. The possible disturbing factor is not the elimination of vision but rather the presence of a bandage.

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[1] With the exception of one group of rats, the experiments were conducted by Mrs. Hicks. The paper has been written by Prof. Carr, who is entirely responsible for the method of treatment and the hypotheses and conclusions advanced.

All four adult subjects and one of the children were familiar with maze experimentation and three of these were very familiar with the rat maze. It was thus necessary to construct the human maze on a plan entirely dissimilar to that of the Hampton Court maze, and yet to preserve a comparable set of difficulties. The Hampton maze presents seven blind alleys, while the human maze was constructed with nine. The individual *cul de sacs* of the rat maze are probably the more difficult. Any comparison of the relative difficulty and complexity of the two mazes is a matter of opinion, though in our estimation the rat maze is the more difficult.

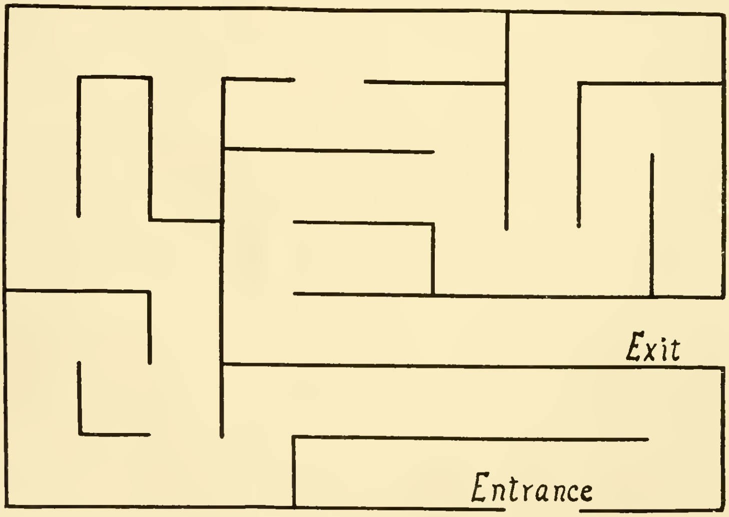

FIGURE 1. Ground plan of the maze used by the human subjects.

The matter of constructing mazes of comparable size presents difficulties. Shall the lengths of the pathways be proportionate to the body lengths, to the length of stride, or to the rate of movement? The results will vary according to the criterion used. The human maze presents a true path of 78 feet with 72 feet of blind alleys as opposed to 40 feet and 32 feet respectively for the Hampton maze. The human maze is thus approximately twice the size of the rat maze, a ratio which is too small according to the length of body or to the length of stride. The mazes are more nearly comparable from the standpoint of the rate of movement. Both human and animal subjects present marked individual differences as to rate of movement. The rats increase their rate of movement during successive trials more than do the humans. In the final trials an average speed of 64, 105, and 79 feet per minute was attained by the adults, children, and rats respectively. Our proportions thus fulfil the conditions of none of these three criteria exactly.

The human maze was constructed in an enclosed yard with a door which was kept locked. The subjects were kept blindfolded during the time they were inside of the yard. Their knowledge of the maze was thus confined to that derived from their experiences in traversing it. To those subjects who possessed no idea of a maze, only the necessary information was vouchsafed, *viz.*, that there was a winding path bounded by wires with only one outlet, and that they were expected to keep moving until this egress was found. They were instructed that success would be evidenced by finding themselves in the open untrammeled by wires, and that the experimenter would call to them if necessary when success was attained. No directions as to method of learning were given, each subject being allowed to learn the maze by his own natural method. Our purpose here was to avoid all constraint; freedom and ease are essential in order to maintain conditions comparable to those obtaining for the animals. The novelty and puzzle character of the task, and the knowledge that other subjects were learning the maze furnished sufficient motives for good records. Introspective analysis was neither demanded nor encouraged. The object was to eliminate as much as possible all memory and study of the maze between trials, so as to approximate conditions which may be legitimately ascribed to the rats. Introspection would tend to fix certain details more firmly in mind and encourage recall between trials. Some introspective knowledge was gained from the subjects' spontaneous remarks. The subjects were introspectively quizzed after the maze was learned. This mode of procedure eliminated the possibility of obtaining some very interesting and important knowledge of the learning process, but our interest was centered, however, upon the objective records obtained under comparable conditions.

We secured records from five children with ages ranging from 8 to 13 years. One of these was very familiar with the Hampton maze and with the rat experiments. The remaining ones were entirely ignorant of the experiment. Four adult males were used. These were all graduate students. Two of these had a practical knowledge of animal experimentation, while the knowledge of the other two was general in character. Records were taken for time, error, and the total distance traversed. A runway was regarded as the unit of error and returns over the true path as well as entrances into *cul de sacs* were regarded as errors.

Comparisons will be instituted between the records of the human subjects and the data derived from two sets of rats run on different mazes and by different experimenters: (1) A group of seventeen animals tested by Mrs. Hicks on the Hampton Court maze and (2) a group of six rats tested by Prof. Carr on a maze with a true path of 30 feet complicated by nine *cul de sacs* whose combined length is 14 feet. The second maze is hardly as complex as the Hampton, and it appears more comparable in complexity to that used by the human subjects. The groups of animals will be termed R I and R II.

I. THE NUMBER OF TRIALS NECESSARY TO LEARN THE MAZE

No correlation can be made between degrees of intelligence and the number of trials. The average and average variation for the adults and children are 11.25 ± 3.9 and 11 ± 2 respectively. The same values for R II are 12 ± 3.6. The individual records for the group R I on the Hampton maze were not preserved by Mrs. Hicks, so that comparisons in this respect are impossible. Miss Vincent, however, has kindly furnished the individual records of 10 animals on the Hampton maze which gave an average learning time of 11.4 ± 4.1 trials.

Mrs. Hicks's criterion of mastery for the human subjects was the ability to traverse the maze once without error, which is undoubtedly too low. The tests should have been carried further. Two successive trials without error were demanded

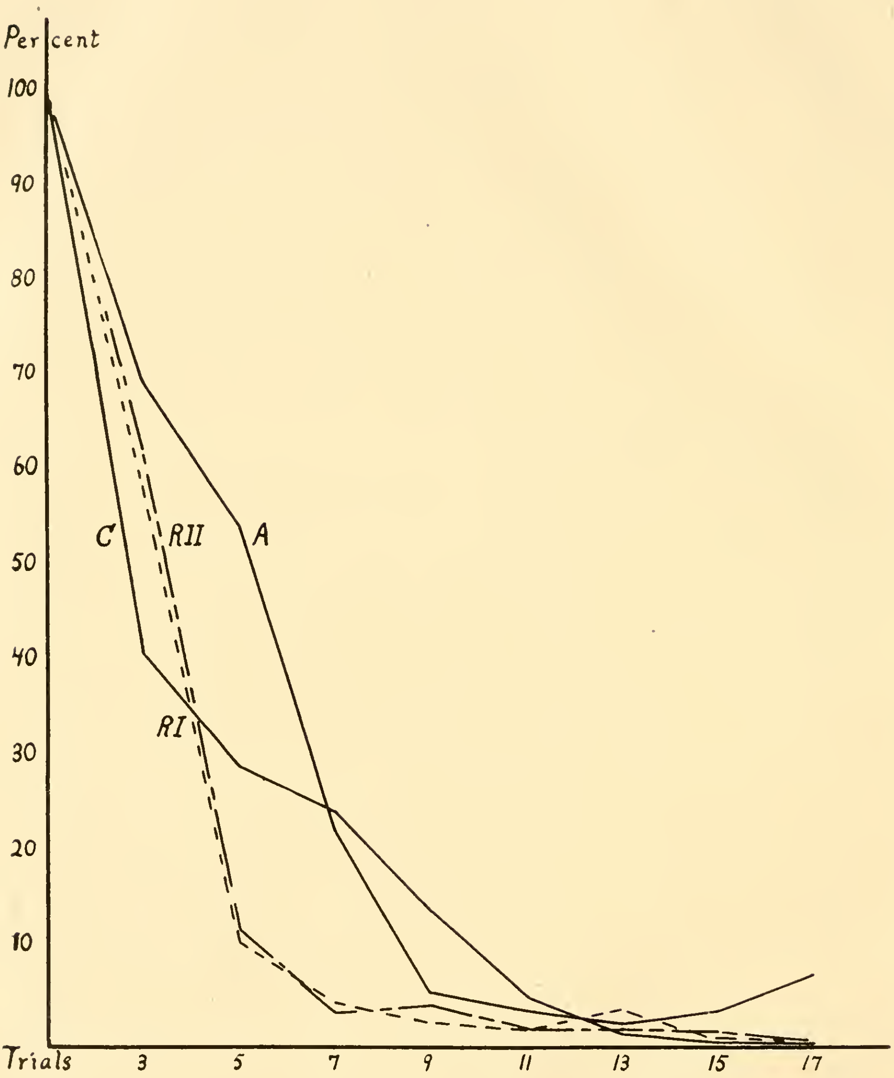

FIGURE 2. Curves representing in percentage terms the elimination of surplus time. A, curve for adults; C, curve for children; R I and R II, curves for the two groups of rats.

of both groups of rats. If but one successful trial had been demanded of the group R II, their results would be 7 ± 3. The values for Miss Vincent's group are 12 ± 4.6 when the criterion of mastery is the ability to run 8 trials out of 10 without error.

While the results indicate that the animals can learn a maze in a less number of trials than human subjects, yet one feels some hesitation in advancing such a conclusion in view of (1) the differences between the mazes, (2) the different criteria of mastery used, and (3) the small number of individuals constituting each group. One is certainly justified in concluding that no correlation exists between the number of trials necessary to learn a problem of this sort and the degrees of rational ability or of the general intelligence of the groups compared. This means that one cannot estimate correctly the relative intelligent ability of any two groups of organisms wholly in terms of the relative number of trials utilized in learning a maze. The conclusion does not mean that there is necessarily no causal relation between the two factors, but that if it does exist, it is obscured by more important factors.

II. GROSS VALUES ELIMINATED

A pronounced and unambiguous inverse relation obtains between the degrees of intelligence and the amount of surplus values eliminated. In a general way the ratios between the values eliminated are approximately 1:2:3 for the adults, children, and rats, respectively. Stated in terms of the usual curve of learning, these figures mean that the average height of the rat curve above the base line is three times as great as that of the adults, while the adult curve would possess but one-half the height of the children's curve.

Tables 1, 2, and 3 give the average surplus time, error, and distance values for the four groups for the successive trials. At the bottom of each column is given the average value eliminated per individual in the learning process, and the average individual amount for each trial. Referring to Table II, column I, it is seen that on the average each adult made 58 errors in learning the maze, or on the average 3.4 errors on each trial.

The surplus values for time and distance are computed by subtracting from the records of each trial the corresponding value for the last trial. If a rat traverses 90 feet in any trial,

TABLE I

Surplus Time Values in Minutes

Trial	Adult	Child	Rat I	Rat II
1	4.09	8.40	21.50	19.80
2	4.22	2.03	15.00	19.74
3	1.49	4.98	10.20	5.15
4	.76	2.10	3.50	3.42
5	3.70	2.77	1.20	1.43
6	1.16	3.04	1.50	.77
7	.67	1.04	.40	.57
8	.27	.98	.49	.35
9	.20	1.39	.61	1.23
10	.10	.64	.38	.21
11	.22	.27	.40	.52
12	.11	.06	1.26	.22
13	.07	.16	.29	.47
14	.15	.06	.18	.38
15	.15	.01	.22	.12
16	.16	.06	.11	.13
17	.45	.00	.05	.00
Total,	16.70	28.00	57.30	54.51
Average,	.98	1.65	3.37	3.20

TABLE II

Surplus Errors

Trial	Adult	Child	Rat I	Rat II
1	10.25	34.8	63.0	44.0
2	15.25	8.8	52.5	37.6
3	5.00	18.0	42.5	17.5
4	2.25	11.0	22.0	22.6
5	6.50	8.8	12.4	10.6
6	4.50	13.0	13.0	3.0
7	2.50	6.0	13.5	4.8
8	1.75	6.4	5.2	2.1
9	1.50	5.4	7.5	10.6
10	1.50	5.4	5.0	2.1
11	.50	.8	5.0	2.6
12	1.00	.4	10.0	.6
13	.75	.8	6.0	1.5
14	1.00	.4	6.0	2.6
15	1.00	.6	8.0	.6
16	1.50	.4	4.5	.6
17	1.00	.0	2.0	.2
Total,	58.0	120.0	278.0	163.6
Average,	3.4	7.1	16.3	9.6

and the length of the true path is but 40 feet, it is obvious that the animal has traversed a surplus distance of 50 feet. If the animal expends 2.50 minutes in any run, while the maze can be traversed after it is mastered in .30 minutes, a surplus of 2.20 minutes has obviously been expended in that trial. The values represent the surplus or unnecessary time or distance utilized in traversing the maze: they represent the effort or activity which is *eliminated* by the learning process, and not the effort or activity necessary to run the maze when learned. Obviously, all errors are unnecessary or surplus in this sense, and the error data of Table II are those commonly given.

There is but one exception to the inverse ratio. The distance values for the rats are less than those for the children, although much greater than those for the adults. This exception is readily explicable when the pronounced difference in the lengths of the two mazes is taken into consideration.

The ratios of 1:2:3 representing the values for the three groups were obtained by computing the ratio among the groups for the total time, error, and distance values separately and averaging the three values for each group.

The differences in the gross values are so great in proportion to the average variability (Table V) of each group, and their inverted relation to the general and rational ability of the three groups is so obvious, that one cannot refrain from concluding that some intimate causal relation exists between intelligent ability and the surplus values.

TABLE III

SURPLUS DISTANCE IN FEET

Trial	Adult	Child	Rat I
1	109.	540.	195.
2	138.	102.	165.
3	37.	99.	135.
4	8.	99.	65.
5	59.	105.	38.
6	37.	117.	45.
7	8.	99.	45.
8	30.	48.	22.
9	9.	60.	26.
10	6.	32.	14.
11	1.	10.	14.
12	1.	5.	20.
13	7.	5.	11.
14	7.	6.	10.
15	7.	2.	7.
16	8.	2.	5.
17	9.	0.	3.
Total,	480.	1351.	820.
Average,	28.2	79.5	48.3

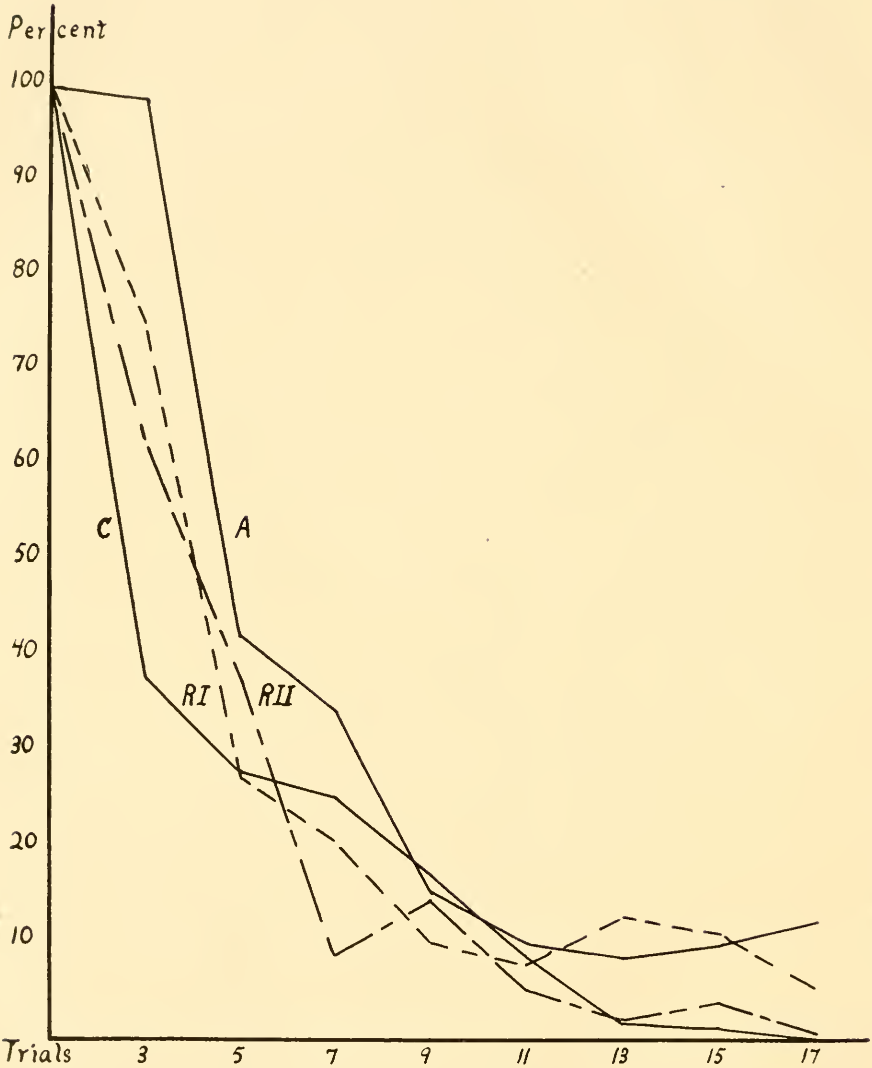

FIGURE 3. Error Curves; representing in percentage terms the rate of elimination. A, curve for adults; C, curve for children, and R I and R II, curves for rats.

III. RELATIVE RATE OF ELIMINATION DURING LEARNING PROCESS

The rapidity of elimination of the gross values is much greater during some parts of the learning process than during others. The extremely rapid initial descent of the curves representing animal learning is a well known phenomenon. The purport of this section is a comparison of the three groups in this respect. The comparison is facilitated by a translation of the gross values into percentage terms. By dividing the value of each trial by the value of the first trial, a series of percentage results is secured decreasing from 100 to zero. The value for any trial indicates the percentage of the initial value yet to be eliminated. These percentage values for the several groups are given in Table IV. The values for time, error, and distance for each trial are averaged, giving a set of results for each group of animals which represents a combination of the three criteria of time, error, and distance. Since all initial values are 100%, this method allows a superposition of curves, a device which renders comparison easy. In constructing the curves from these tables, we have averaged all results except for the first trial by twos, in order to eliminate extremes of variability. These curves are represented in Figures 2, 3, and 4. Several facts are obvious.

1. All curves exhibit the rapid initial descent. In a general way, 85% of the values are eliminated during the first half of the learning process. Obviously, this extremely rapid initial elimination cannot be interpreted as due to the functional efficiency of rational processes.

2. In the character of this initial elimination, certain differences obtain between the three groups for all the curves, and the rats represent a mean between the two extremes represented by the two human groups. The children make the most rapid initial elimination; the rate of the adults is the slowest, while both sets of rats occupy a mean position. But an inverse relation obtains between the rapidity of elimination and the number of trials over which it is sustained. The rate for the children is the most rapid, but it persists for the shortest time (3–4 trials). The rate for the adults is the slowest, but it persists for the longest time (up to the 9–10 trial). The initial spurt of the rats terminates around the 5–7 trials.

Since these differences are relatively pronounced, characterize all three sets of data, and apply to two sets of rats run by dif-

TABLE IV

Percentage Rate of Elimination

Trials	Adults				Children			
	Time	Errors	Dist.	Ave.	Time	Errors	Dist.	Ave.
1	100.	100.	100.	100.	100.	100.	100.	100.
2	103.	148.7	129.	126.9	24.	25.	19.	22.6
3	36.3	48.7	34.5	39.8	59.	51.	18.	42.6
4	18.5	21.9	7.5	15.9	25.	31.	18.	24.6
5	90.2	63.4	55.1	69.6	33.	25.	19.	25.6
6	28.3	43.9	34.5	35.9	36.	37.	21.6	31.5
7	16.3	24.3	7.5	16.0	12.4	14.	18.	14.8
8	6.6	17.	28.	17.2	11.6	18.	9.	12.8
9	4.8	14.6	8.4	9.2	16.5	15.4	11.	14.3
10	2.4	14.6	5.5	7.5	7.5	15.4	6.	9.6
11	5.3	4.8	.9	3.6	3.2	2.3	1.8	2.4
12	2.7	9.7	.9	4.4	.7	1.1	.9	.9
13	1.7	7.3	6.5	5.1	1.9	2.3	.9	1.7
14	3.6	9.7	6.5	6.6	.7	1.1	1.1	1.0
15	3.6	9.7	6.5	6.6	.1	1.4	.4	.6
16	3.9	14.6	7.5	8.6	.7	1.1	.4	.7
17	10.9	9.7	8.4	9.6	0.	0.	0.	0.

Trials	Rat I				Rat II		
	Time	Errors	Dist.	Ave.	Time	Errors	Ave.
1	100.	100.	100.	100.	100.	100.	100.
2	69.7	83.3	84.6	79.2	98.8	85.4	92.1
3	47.0	67.4	69.2	61.2	25.9	39.8	32.8
4	16.3	34.9	33.0	28.1	17.2	51.3	34.2
5	5.6	19.9	19.2	14.9	7.3	24.1	15.7
6	7.0	20.6	23.0	16.9	3.9	6.7	5.3
7	1.8	21.4	23.0	15.4	2.9	10.9	6.9
8	2.3	8.3	11.5	7.4	1.8	4.7	3.2
9	2.8	11.8	13.0	9.2	6.2	24.1	15.1
10	1.7	7.9	7.2	5.6	1.1	4.7	2.9
11	1.8	7.9	7.2	5.6	2.6	5.9	4.2
12	5.9	15.8	10.2	10.6	1.1	1.3	1.2
13	1.4	9.5	5.9	5.6	2.4	3.4	2.9
14	.9	9.5	4.8	5.1	1.9	5.9	3.9
15	1.0	12.7	3.9	5.9	.6	1.3	.9
16	.5	7.1	2.6	3.4	.7	1.3	1.0
17	.2	3.1	1.8	1.7	0.	.4	.2

ferent experimenters on two different mazes, one is forced to conclude that they represent genuine differences in the learning processes of the three groups. Moreover, since the rats stand midway between the two human groups, it is obvious that no correlation exists between the character of the initial elimina-

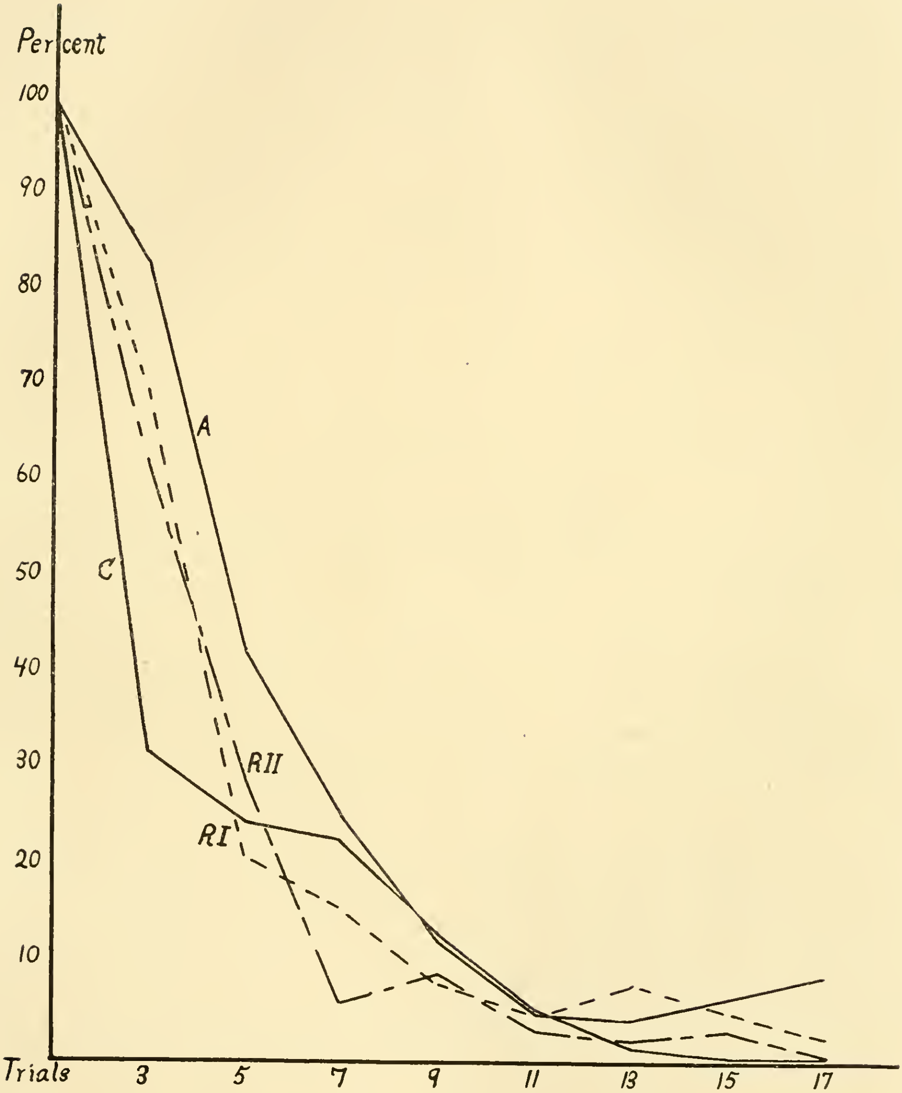

FIGURE 4. Combination Curves; representing the rate of elimination in percentage terms; obtained by averaging the data for error, and surplus time and distance. A, curve for adults; C, curve for children; R I and R II, curves for rats

tion and the rational ability of the organism. This does not necessarily mean that no causal relation obtains between rational ability and the initial progress in learning. If any causal relation obtains, it is indirect and obscured by other factors. One can merely conclude from these facts that one can not infer the relative intelligent ability of any two groups of organisms from this particular characteristic of their learning curves.

3. During the rapid initial spurt, the rate of elimination is not uniform. During some trials the elimination is more rapid than at other times. Certain differences between the groups exist in this respect. In every case the most rapid elimination occurs for the children during the 1–3 trials. For the adults this period occurs later in the learning process, between the 3–5 or 5–7 trials. The rats again occupy an intermediate position, though the curve is more similar in this respect to that of the adults than to that of the children. The most rapid elimination occurs generally from the 3–5 trials, though sometimes during the second and third trials. Again no correlation obtains between the phenomenon and the relative intelligence of the groups.

4. The children's curves invariably exhibit some indication of the plateau phenomenon from the third to the seventh trials. Some slight indication of plateaus are exhibited by the adults, but only for the error and distance values. The rats' curves give the least indication of this characteristic.

IV. COMPARISON OF TIME, ERROR AND DISTANCE ELIMINATION

The relative rate of elimination of time, error, and distance for any group might be shown by plotting the percentage values of Table IV. The facts can better be exhibited by the following plan. In comparing time and error, for example, the number of errors for each trial is divided by the number of minutes for the corresponding trial. This gives a series of values representing the number of errors made per unit of time for the successive trials. The ratios between these values are determined with the initial value regarded as unity, and these ratios are plotted in a curve. Any rise in this curve means that for that period the time values have been eliminated faster than those for error. A fall of the curve means that the errors have been decreased more rapidly than the time. The degree of difference in the elimination is represented by the height of the rise or fall. These curves for the various groups are given in Figures 5, 6 and 7. Each figure compares two sets of values, time and error, time and distance, and error and distance. In each couplet one factor has been regarded as the unit of comparison, and this is printed first in the heading. Time is the unit in two comparisons, and error is the unit in the third. This fact is important, from the standpoint of interpretation, because a rise in the curve means that the first or unit factor is decreased more rapidly than the second factor at that point in the learning process. With this key one can readily read the meaning of the curves in terms of relative rate of elimination.

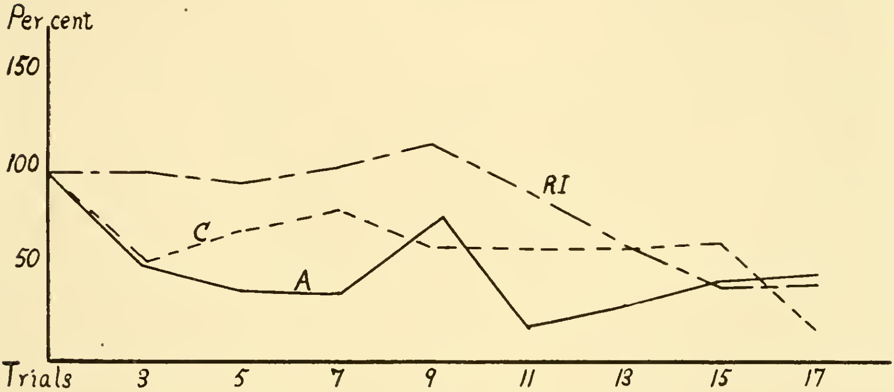

FIGURE 5. Error and Distance Comparison. Graphs representing the comparative rapidity of the elimination of error and distance values. A fall of the curve indicates that surplus distance has been eliminated faster than errors. A, adult curve; C, children's curve; R I, animal curve.

1. Error and Distance (Fig. 5). (a) For the entire learning process up to the 17th trial, all three groups eliminate distance more than errors and to about the same degree. The groups differ mainly as to the *period* in which this greater elimination occurs. (b) For the animals it occurs during the second half of the learning. The level line indicates that error and distance were eliminated at practically the same rate up to the ninth trial. The humans, on the other hand, perform this more rapid elimination of distance during the first part of the learning. (c) In a general way, the groups may be arranged in order in this respect similar to their order as to intelligent ability. The

curves of the adults and rats occupy the two extremes. The curve of the children is a mean, but it approximates more closely the adult curve than it does that of the animals.

The independent variability of error and distance can have but one meaning, when one considers the unit of error adopted in this experiment. Entrance into an alley constitutes an error, irrespective of the distance entered. At first the alleys are traversed throughout their entire length. In the later trials, the alley is only partially explored, this being sufficient to identify the runway as a mistake. One error is counted in each case, but the distance traversed is much smaller in the second

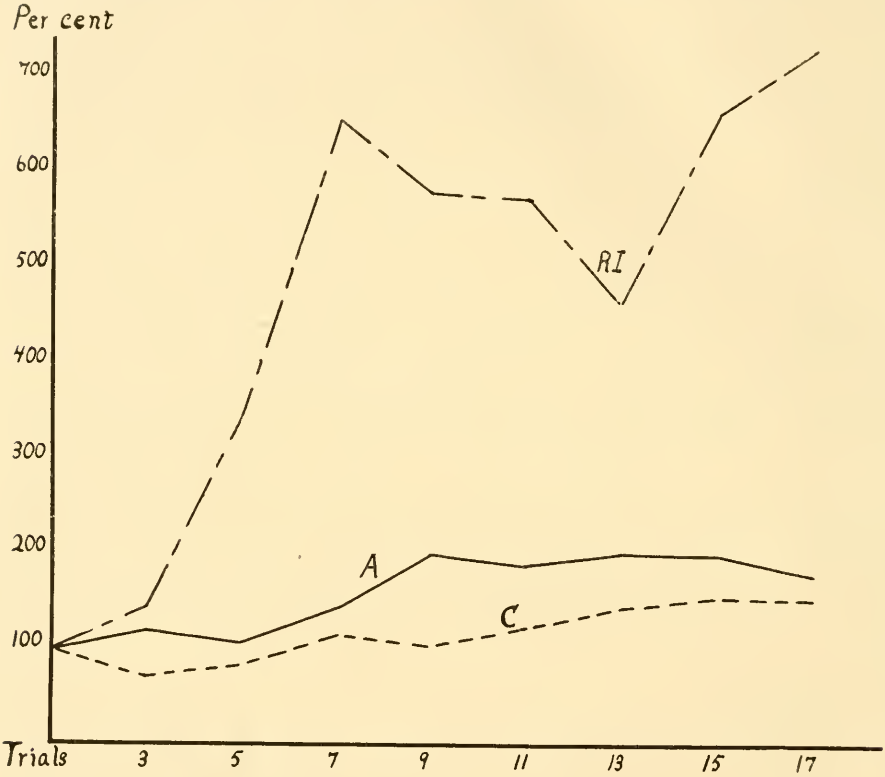

FIGURE 6. Time and Distance Comparison. Graphs representing the speed of running, or the rate at which distance is decreased in relation to time. Rise of curve indicates that time has been decreased faster than distance. A, adult curve; C, children's curve; R I, animal curve.

trial, i.e., the distance values have been decreased while the error values have not. The fact that all groups eliminate surplus distance values faster than errors thus indicates that at first they tend on the average to explore every runway throughout its length, while later they can identify a wrong runway by a partial entrance. This interpretation brings the comparative facts into some causal relation to intelligent ability, because the animals are not on the average able to perform this feat successfully until the latter part of the learning process, while the adults achieve the result mainly during the first few trials. Again, the adults are more successful than the children during this initial period.

2. Time and Distance (Fig. 6). In comparing time and distance, we are dealing with the factor of the speed of running for the various trials, i.e., the distance traveled per unit of time. (a) The average rates of speed of the adults, children, and animals, for the total learning process are 59, 79, and 52 feet per minute, respectively. The rats thus travel on the average almost as rapidly as do the adults. In fact they are much speedier in the final trials. (b) As to increase of speed, the human groups are approximately similar, while the rats stand in a distinctive class by themselves. They increase their speed seven fold. Some increase in speed is characteristic of all groups. The order of the three groups is not the same as that of their relative intelligence.

Obviously the facts are not explicable in terms of intelligence. The differences can better be explained in terms of peculiarities of the initial trials. The behavior of the rats is dominated at first by motives of caution and curiosity. A specific food motive is necessarily lacking. Slow, cautious, and random activity characterizes the first trials, and the speed of running is immediately increased with the introduction of the proper incentive. The incentive for rapid movements exists for the humans at the beginning. The children start out with a high degree of confidence and assurance, and run rapidly. In succeeding trials, they are forced by their blunders to run more slowly and cautiously (witness the initial drop of the curve), while the speed is again increased as mastery of the maze brings confidence and assurance. The adults proceed at the start with some dignity and circumspection and, as a consequence, their speed increases in a more regular manner. The initial values for the rats, adults, and children are, respectively, 10.5, 36.3, and 67.5 feet per minute. The final values in the same order are 79, 64, and 105 feet per minute.

3. Time and Error (Fig. 7). We find a pronounced distinction between the two groups of rats on the one hand and the human groups on the other. The animals eliminate time relatively faster than they do errors, while the human subjects eliminate errors more rapidly than time. The human curves are very uniform, while those of the animals are highly irregular. The period in which the greater elimination of errors mainly occurs for the human subjects is the second half of the learning process. The emphasis upon time by the animals occurs during the earlier trials. During the later trials, the animal curves fall in an irregular manner. That is, during the latter half of the learning, the animals eliminate errors faster than time and at about the same rate as do the human subjects. The main difference between the curves seems to be the matter of speed. The initial time values of the animals are exceedingly high,

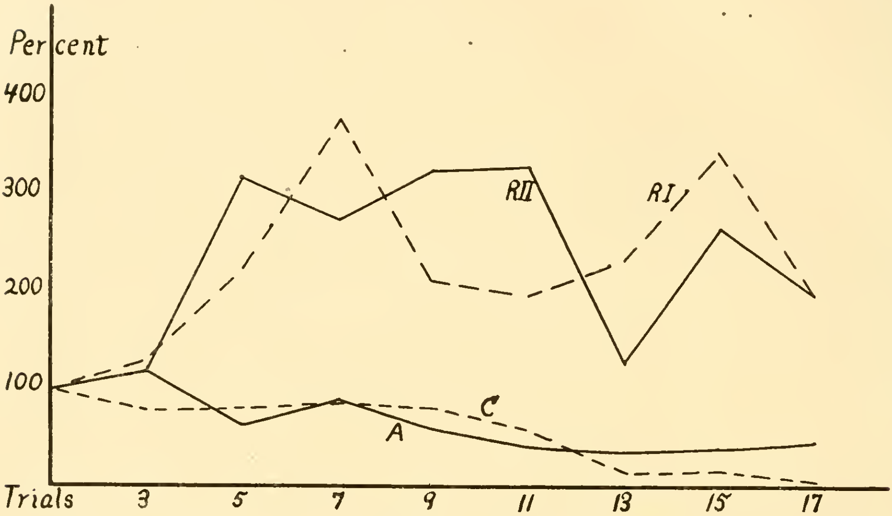

FIGURE 7. Time and Error Comparison. Graphs representing the elimination of errors in relation to that of time. Rise of curve indicates that time has been decreased faster than error. A, adult curve; C, children's curve; R I and R II, animal curves.

due to fear and caution and an absence of specific motive for a high rate of speed. After a few trials the time values are decreased, and from this point on the relation between time and error is more normal and similar to that exhibited by the human subjects.

V. RELATIVE VARIABILITY OF THE GROUPS

Comparisons in this respect were instituted in the following manner. The average and average variation values were computed for each group for such factors as the total number of errors, the surplus time, etc. Inasmuch as the rats made a much greater number of errors than the adults, the range of probable variation is also greater. Consequently, each average variation is divided by the average. This gives a percentage value expressing the ratio between the range of variation and the group average. These values have been termed the "percentage of variability," and they are given in Table V.

TABLE V

Percentage of Variability

	Adults	Children	Rat II
Initial Errors	15.6	19.8	57.5
Initial Surplus Time	34.6	32.4	41.8
Total Errors	26.7	20.7	29.0
Total Surplus Time	23.4	57.4	24.8
Number of Trials	34.7	18.2	30.0
Average	27.0	29.7	36.6

The average for each group is given at the bottom of the table and it is to be noted that the groups can be arranged in order of their intelligence. One might conclude, therefore, that the greater the intelligence the less is the variability between individuals. However, it must be recalled that four adults, five children, and six rats constituted the three groups. The probable range of variation inside a group depends, in a general way, upon the number of individuals composing it. Making allowances for this fact, no distinctive differences can be confidently affirmed, with the possible exception that the animals are more variable than the humans as to the initial values.

VI. RELATION OF THE LEARNING CURVES TO INTELLIGENCE

Our results indicate that the two features of any set of results which serve best as the main criterion or index of the intelligent capacity of a group of animals are the gross values eliminated during learning, and the rapidity with which errors are eliminated relative to surplus distance.

This conclusion is interesting because it runs diametrically opposite to current theory and practice. There have been in the main two assumptions as to the relation between a learning curve and the intelligent level of a group.

1. Both comparative and human psychology have tacitly assumed, at least so far as practice is concerned, that the rating of any two groups as to general intelligence is to be based solely and directly upon the number of trials involved in mastering a problem. This assumption is perhaps valid in memorizing nonsense material in such experiments as the Ebbinghaus type, in which the various trials are relatively homogeneous as to the amount of effort expended. In problems of a motor character (and this applies to all animal experiments and to much human work) the various trials are so radically disparate as to the time and effort expended that the assumption is without any rational basis. Moreover, the assumption is controverted by the facts of this paper. However, the inference must not be made that no causal relation obtains between general intelligence and the number of trials. We object to relying upon this feature as the sole or main criterion in rating general ability.

The position may be taken that the number of trials may be regarded as an accurate index of *special* ability—the ability to learn the maze problem. The writer is frankly skeptical of such an assumption although the point is irrelevant, for comparative psychology has not been using the criterion in this manner. It has been rating groups of animals as to their order in the intelligent scale, and this means general and not special ability.

2. Comparative psychology has also broached the proposition that the *rational* status of a group of animals can be legitimately inferred from such a feature as the " slope " of a learning curve. This assumption does not refer to general intelligent ability, but to *rational ability*.

Thorndike assumed that a *gradual slope* of a time curve is indicative of the absence of higher mental processes. The functional presence of rational processes would be evident in an abrupt descent. Hobhouse, while not explicitly accepting the assumption, called attention to the abrupt initial descent of Thorndike's curves and maintained that, according to the assumption, rational processes were functionally present. Watson questions the assumption and argues that this abrupt initial drop of learning curves can be legitimately explained in other terms.

The slope characteristic of any curve is determined by three factors which are to some extent independent variables. These are (a) the number of trials, (b) the surplus values, and (c) relative rate of elimination for different trials. Obviously, the slope of a curve increases as the number of trials decreases, when the other two factors remain constant. The abruptness of the slope is also directly proportionate to the size of the gross values, when the other factors remain constant. Likewise, two curves may be alike as to the gross values and the number of trials, and yet their slope may be radically different; one may exhibit a straight line descent, while the abrupt initial drop may characterize the other. Obviously, Hobhouse and Watson are discussing this " abrupt initial descent " as compared with the remaining portion of the curve, and this is the third factor mentioned, viz., the relative rate of elimination. Hobhouse assumes that Thorndike also referred to this feature of the curve. Thorndike's term " gradual slope " is to some extent ambiguous and the writer has often harbored the suspicion that the emphasis in Thorndike's mind was placed upon the " number of trials " rather than upon the " relative rate of elimination." Obviously a learning process which extends over twenty trials may be termed " more gradual " than one which terminates in four trials.

Our results indicate that any inference from such a general characteristic of a curve is not feasible, because we are dealing with a complex phenomenon due to several independently variable factors. Our results indicate that the rational status of a group of animals cannot be inferred from the slope of a curve in so far as this slope is dependent upon the number of trials or the relative rate of elimination. They indicate, moreover, that inferences as to intelligent status are legitimate in so far as the slope is determined by the factor of total values eliminated, but that the relation between the abruptness of slope and the degree of rational ability is just the inverse of that assumed by Thorndike and Hobhouse.

Several hypotheses may be advanced to explain the inverse relation between intelligent capacity and the gross values.

1. Differences in previous knowledge of the maze. Our experiment failed to establish comparable conditions for the three groups in the matter of previous knowledge of the problem. The humans knew the meaning of the problem; they knew that the maze was constituted of blind alleys and a true path with an exit. Their acts were purposively controlled by the motive of finding this exit as soon as possible. They were governed by no motives of curiosity or fear leading to idle and unnecessary exploration. The rats face a different situation. There is no distinction between blinds and true path. To them, there is no exit leading to food. The maze as an object from which to escape as quickly as possible constitutes a problem which the animals must discover by experience. Motives of fear and curiosity leading to random exploration are dominating factors which must be eliminated in the early trials. The motive of hunger at first stimulates but does not direct their exploring activities toward some exit. To the rats the maze at first is a strange, novel object to be explored and investigated in all its details, while to the human subjects it is primarily an object from which to escape as soon as possible. The rats proceed slowly and cautiously, while the humans travel with some speed and assurance. The human explorations are systematized, while those of the animals are random and discursive from the standpoint of reaching some definite position in the maze. One set of conditions tends to increase the initial values while the other set tends to minimize them.

Undoubtedly this conception is legitimate and the phenomenon is to be explained at least in part by such means. Two considerations may be urged against the conception as the *sole* principle of explanation. (a) The differences between the three groups as to previous knowledge of the problem is utterly disproportionate to the corresponding differences of surplus values. (b) If the differences in total values are to be explained entirely in such terms, then all three groups would make the same number of errors in learning the maze, provided they could start on equal terms. Making comparisons in terms of errors, group R I would be on terms of equality with the children and adults only after three and seven trials, respectively. If it were not for this handicap equivalent to a number of trials, the animals would learn the maze in a much fewer number of trials than the adults. The conception as the sole principle of explanation thus forces the further conclusion that the number of trials necessary to learn a maze is inversely proportional to general intelligence.

2. The second hypothesis concerns the functional efficiency of a *plan of action.* Let us suppose that a subject explores both sides of each runway carefully seeking for two diverging paths. He chooses one of these in a haphazard manner for exploration. The exploration is designed to determine whether or not this path is a blind alley. If by chance he choose the true path, he continues until he again comes to the parting of the ways. In case he chooses a blind alley, his mental effort is now devoted to the task of schematizing its spatial relations to the path over which he came and to the other diverging path. His purpose is to emerge from each alley that proves to be a blind and to continue on his way without any disturbance of orientation. Provided this result were attained in every case, the average number of initial errors for a group of subjects would approximate half the number of blind alleys in the maze. If the subject failed to maintain his orientation upon emerging from a blind alley, returns over the true path, and re-explorations of *cul de sacs* would result with a consequent increase in the time and error values. This ability to reinstate the previous sense of orientation upon emerging from a blind alley is no easy task. It involves during the exploration the construction of some representation of the spatial relation of the runway to other paths, and the mental ability to hold this scheme in mind until the previous orientation has been reinstated. The successful prosecution of such a plan of action does not necessarily result in the mastery of the maze during the first trial, because the subject is required to schematize and hold in mind but one segment at a time. The first segment learned may be forgotten long before the end of the maze is reached. The subject may also be unable to unite the various parts into a conceptual whole, and, granted the construction of a schematic

representation of the whole path traversed, it may not be remembered upon the second trial on the following day. This description of a possible plan of procedure is designed merely to illustrate our general proposition that any systematic plan of action must necessarily be effective upon the records of the trials, and that its effectiveness is proportionate to the character of the plan, to the mental ability of the subject, and to the persistency with which the plan is prosecuted.

This hypothesis is similar to the first in so far as the plan of action is derived from previous knowledge of the maze. However, we desire to emphasize several points. Two groups might be on a par so far as previous knowledge is concerned and differ immensely in the ability to use it effectively. One group might be unable to evolve an adequate plan, their judgment as to its adequacy might be poor, and they might be weak as to the virtues of persistency and pertinacity. Again, the development and choice of plans or modes of attack may depend not only upon the previous knowledge of the maze problem but upon experiences derived from widely disparate sorts of situations.

3. A third hypothesis assumes the efficacy of acquired habits and innate tendencies. Practically, the hypothesis is the same as that of purpose or plan, with the exception that the determining conditions are conceived as having no conscious correlate. Children are characterized by sensorial attention and curiosity. In popular terms they are all eyes and ears. Their nervous system is highly responsive to all sorts of objective stimuli; their reactions are determined mainly by objective conditions; relatively, they are creatures of the environment. Adults are more blase; they do not give themselves up to every chance novel stimulus; their reactions come to be more and more determined by acquired internal neural conditions; they are masters of, rather than slaves to, their environment. Psychology is accustomed to emphasize the positive phase of the selective character of habit in apperceptive reactions to sensory stimuli. Habit is also selective in a negative sense; it curbs or represses many of the original tendencies to response; it results in the neglect on the part of the organism of many seductive appeals to reaction. The general point to be emphasized is the fact that children are extremely sensitive to a wide range of stimuli, With accumulated experiences, adults become neglectful of

those stimuli which they have learned are probably of an inconsequential character. The wider the experience of an individual in the world, the more blase, as a rule, he is likely to be.

The same conception is applicable to animals. The behavior of the young is characterized by an almost feverish, irrepressible exuberance of activity of a highly random, impulsive, and purposeless character. Mature animals act, as a rule, with some degree of dignity, caution, and circumspection. They have rather thoroughly explored their environment and classified it in reactive terms. Their habits on the one hand have increased their sensitivity to certain stimulative features as objects of food and danger. On the other hand, habit has also curbed and repressed responses to the trivial and inconsequential aspects of the environment. As a result their energy is better directed and conserved for the more immediate and practical needs of the organism. The difference refers not only to the amount of activity, but also to its character.

This conception is applicable to differences between species, e.g., the comparison of the rats and the human subjects in our experiments, as well as to differences between adults and young of the same species. An organism with the greatest capacity for intelligence is, other things being equal, the most likely to acquire the greatest number of these inhibitory habits, and, as a consequence, is the least likely to give free rein to its discursive and random impulsive tendencies.

This view suggests the general proposition that the functional presence of higher mental processes inhibits and supplants the " motor excess " characteristic of the trial and error method of learning. Without the higher processes, the organism must rely for success upon the number of random movements; when conduct becomes guided more and more by higher processes, the extreme tendency to " motor excess " is not only useless but disturbing to the operation of the rational activities. This suggests that the degree to which organisms rely upon and utilize the trial and error method (excessive explorations) of learning may be determined by innate tendencies as well as by acquired habits and rational processes. An organism with little capacity for the higher sorts of intelligence, is necessarily constructed on the plan whereby this " motor excess " feature of behavior is accentuated. With organisms of higher mental capacity, this peculiarity of behavior is not necessary and, as a consequence, this tendency is repressed.

The relative validity and importance of these three explanatory concepts is immaterial to this paper. We are interested in maintaining the reasonableness of the assumption of a causal relation between intelligence and the surplus values. The proposition that the total values constitute the best single index of intelligent capacity is also rational from another point of view. The surplus values eliminated represent the amount of exploration, or the amount of motor effort, necessarily involved in the mastery of the problem. It is a customary assumption that the ability to master a task is inversely proportionate to the effort necessarily involved, and that the quantitative estimations of the amount of effort expended serve as the best criterion for judgments of comparative ability. In memory experiments of the Ebbinghaus type, the conditions are so arranged that the comparative amount of work expended by two subjects is best expressed by the "number of repetitions or trials." The same procedure would also apply to problems of the maze type, *provided* all trials were equal as to the effort involved. Obviously, the trials are utterly disparate in this respect and as a consequence the "total values" is the only aspect of the curves which represents in any adequate manner the factor of "effort." In this connection it is well to note that the "total values" also represent in a way the "number of trials," for, to a certain extent, the surplus values must vary with the number of trials.

The factor of "surplus values" cannot, however, be regarded as a very accurate or delicate index of intelligent ability. (a) While it is the only representative of the "amount of effort expended," yet it can not be considered as an adequate representative. The time and error values are directly proportionate to the *motor* effort, but their relations to any effort of an intellectual sort is more complex. The time values may be regarded as measuring the amount of mental work so far as its duration is concerned, but, as we have noted, the two are *inversely* related in respect to the *degree* or *efficiency* ofthe mental processes. (b) When the criterion is based upon the time and error values, the ratios between the three groups are

1:1.9;3.6. That these ratios express the relative ability of the three groups with accuracy is a doubtful proposition. The high degree of variability inside of each group must also be considered. The percentage of individual variability for time and error (Table V) is approximately 30%, for each group. It is doubtful if the intelligent capacity of the individual members of any group varies to any such extent. When corrections are made for the average variability, the above ratios should read 100 ± 30:190 ± 57:360 ± 108. Such values can express the relative status of the groups only in a gross and general way. (c) We have expressly refrained from maintaining that no causal relation obtains between intelligent ability and the "number of trials" and the "rate of elimination." If the "surplus values" represent the "amount of effort," the "number of trials" and the "rate of elimination" can be embraced under the caption of the "distribution of effort." Experiments on human memory have demonstrated that the "distribution of effort" is a very essential factor in economic learning. Preliminary experiments in this laboratory have also demonstrated its importance in such a problem as the maze. To maintain that such features should be totally ignored, in interpretations of intelligent ability, is preposterous. Furthermore, the three factors are not independent variables, but they are interrelated in a very complex manner. Intelligence is related to each factor both directly and indirectly. An insistence upon the intricate complexity of the three factors as to their relations to each other and to intelligence leads to several suggestions. (1) It helps to explain why the "surplus values" are not an accurate index of the degree of intelligence. (2) It suggests that an accurate index must be based upon some formula which shall give due weight to the element of "distribution of effort." (3) Comparative psychology has many problems to solve before making any attempt to rate the intelligent status of groups of animals on the basis of their learning curves. As yet practically nothing is known of the real meaning of a learning curve and the interrelations of its several features. (4) The conception renders explicable several peculiar features of our results. For example, there exists an inverse relation between the initial values and the number of trials. In so far as intelligence operates to cut down the values of each trial, it necessarily increases the number of trials. It is probably true that, other things being equal, the human can learn the maze in a fewer number of trials than the rats, but that this direct connection of intelligence with the number of trials is overpowered by their indirect relation through the factor of "total values." This inverse relation between the number of trials and total values is, of course, not absolute; no one maintains that the adults would have learned the maze in one trial provided they had bunched all their fifty-eight errors in the initial attempt. The inverse relation is not absolute *because* the element of "distribution" is an effective factor. In all probability the fact that the rats stand midway between the two human groups as to "rate of elimination" is explicable in some similar manner. The "initial drop" *may* be directly related to the rational processes as is assumed by Thorndike and Hobhouse. Yet, on the other hand, because the rational processes function to prevent any *cul de sac* from being repeatedly explored, they necessarily *prevent* the rapidity of this initial drop. As a consequence, this feature of the curves is related to intelligent ability in two antagonistic ways.

VII. CONCLUSIONS

For the maze problem, there are only two features of the numerical representatives of the learning process which can serve by themselves as direct indices of intelligent capacity. These are the surplus values of time and error, and the rapidity with which errors are eliminated in relation to distance.

These criteria are valid only for certain problems, and their accuracy and delicacy as indices are limited to relatively gross comparisons. Whether they serve best in estimations of rational status, general intelligent capacity, or of the special ability for motor acquisition is largely a matter of opinion.

Any accurate index of intelligent capacity must consist of some formula which shall give due weight to all factors: it must represent the "distribution" as well as the amount of effort. Such a formula must necessarily vary according to the problem.

The complex interrelations of the various features of any numerical representation of a learning process, the organic complexity implied in such a term as "intelligence," and the intricacy of the relations between "intelligence" and these numerical factors can not be too strongly emphasized in view of the tendency frequently manifested to rate animals in an intelligent scale on the basis of experiments in learning.

An experimental analysis of this complex situation constitutes a problem for any psychology which professes as an ultimate aim the ability to grade organisms in an intelligent scale from objective results quantitatively stated.

PHOTOTAXIS IN THE SEA URCHIN, *ARBACIA PUNCTULATA*

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GENERAL FEATURES OF THE REACTION

Arbacia punctulata, like many other species of sea urchins, is quite responsive to light and usually tends to go toward the darkest part of its environment, although more rarely certain individuals may show a pronounced positive phototaxis. This species may be quite fitful, however, in its reactions, and there is a very large amount of individual variability in the phototaxis of different specimens. When the sea urchins are brought near a window from a comparatively dark region they slowly wander away from the light approximately in the direction of the rays. When the dish in which they are contained is reversed they may again go away from the light, but after a few excursions of this kind they settle down and remain quiet. Even strong sunlight fails to disturb them for more than a short time. In fact, specimens remain phototactic in weak light longer than in light of high intensity, which quickly dulls their sensitivity. Specimens which no longer react to weak light may show a negative response of short duration when brought into strong light. Whatever the direction and strength of the light, provided all parts of their enclosure are illuminated to nearly the same intensity, the sea urchins do not remain for more than a limited period in the negative end, but become scattered in entire disregard of the direction from which light falls upon them.

After sea urchins have ceased to respond to the light they may be induced to resume their phototaxis by mechanical stimulation. Poking the spines about or picking up an individual and setting it down again usually starts the creature crawling toward the negative end of the dish. Stimulation by chemicals has the same effect. Since locomotor movements are somewhat unwonted activities under the natural conditions of the sea urchin's life and involve a condition of more or less general excitement, they tend to exhaust themselves rather quickly. The effect of disturbance upon the creature's phototaxis is apparently that of producing a condition of activity which is directed and controlled to a certain extent by the rays of light.

When sea urchins are brought near a source of light there is produced a general movement of the spines and tube feet over the whole surface of the body. The points of the spines move back and forth or describe a more or less circular course, each spine moving in a manner different from its neighbors. The tube feet are extended, retracted and bent in various directions. Soon order begins to emerge out of this chaos of actions. The spines on the side toward the light become depressed and relatively quiet. Those on the opposite side tend to become somewhat raised. The spines on the sides, and especially those that reach the bottom move back and forth, and tend, when they are pulled back, to force the animal ahead. The movement resembles that of a galley rowed by numerous oars. The spines can frequently be seen to be raised somewhat when they are carried forward as an oar is in rowing. There is considerable irregularity in their movements, but there seems to be a predominant tendency to combine a downward movement with the backward phase of the stroke, a circumstance which would easily explain the rôle of the spines in locomotion. As the sea urchin travels the test is raised and lowered at intervals owing to the irregular movements of the spines.

The movement away from the light is aided by the action of the tube feet. Those on the under side of the body are protruded and bent in the direction of locomotion. As they touch bottom they adhere by their terminal disk, contract, and thus pull the body forward. The direction of locomotion is not determined by a particular set of tube feet which lie on the anterior side, because all of these organs capable of reaching the bottom are brought into play. Anteriorly, posteriorly, and at the sides the tube feet as they extend are bent forward and coöperate to pull the animal along very much as they do in the locomotion of the starfish described by Jennings.

Generally also, there is an extreme extension of several of the tube feet from the equatorial or upper region of the corona on the side away from the light. Those on the more illuminated side are more retracted. The extended tube feet sway back and forth slowly like long feelers exploring the region in front of the body.

When light falls on Arbacia vertically and one-half of the test is shaded the animal crawls directly into the shade. If placed where sunlight falls upon it obliquely from above and if either the right or the left half of the body is shaded by a board as the sea urchin crawls away, it travels in the general direction of the rays but deviates to a considerable extent toward the shaded side. If the side toward the sun is shaded and the sunlight allowed to fall upon the other half, the sea urchin will generally go against the rays of the sun toward the shade. The sea urchin, as we might expect, tends to go away from whatever side is the more intensely stimulated.

Occasional individuals show a positive phototaxis under the same conditions in which others are strongly negative. One such individual made six successive straight trips toward the light after which it became indifferent or slightly negative; several others showed a positive reaction which was nearly as persistent. In the positive reactions the spines are bent slightly downward on the side toward the light and are raised on the opposite side; and several of the tube feet from near the equatorial region of the test become greatly extended in the direction of locomotion. In all of the positive specimens examined the rate of movement was slow compared with the negative ones and occurred only in rather dim light. It cannot be said, however, that the majority of specimens observed were positive even in very dim light, although strong light produced a negative reaction in all specimens in which a well marked phototaxis occurred.

Removal of the organs in the ocular plates which have falsely been regarded as eyes produced, contrary to the statement of Romanes[1] for Echinus, not the least change in the reaction to light.

REACTIONS TO SHADOWS

Arbacia reacts to shadows in much the same way as several other species of sea urchins. Pass a dark object over it and the spines are erected so as to stand out at right angles to the surface of the body. This reaction may be repeated three or four times, after which the animal fails to respond. After several minutes of rest the action may be again induced. Keeping individuals in the laboratory for some days diminishes their sensitiveness to shadows, and indeed their reactions to photic stimuli in general.

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[1] Jellyfish, Starfish and Sea Urchins, p. 321.

The reaction to shadows is doubtless of service as a protective device, as it has commonly been regarded. There seems, however, to be no manifest tendency for the spines to turn toward any particular region which is shaded. Small shadows were thrown upon various parts of the sea urchin. The nearest spines would react by becoming more nearly perpendicular to the surface. If the spine happens to point away from the shaded spot it is brought so as to point more nearly toward the object casting the shadow, but if it is pointing toward the shaded spot it reacts by straightening up as before. Horizontal light was thrown on several sea urchins and my hand was passed between them and the light. The spines pointed directly outward instead of toward my hand. To a sudden diminution of light the sea urchin responds by bristling up, regardless of the position of the source of the shadow.

Von Uexküll [2] found that spines on isolated parts of the test provided with a radial nerve would still show the shadow reflex, but that this reflex would no longer occur when the radial nerve was destroyed. I have repeated von Uexküll's experiments and obtained the same results.

LOCAL REACTIONS OF PARTICULAR PARTS

As we have seen, local shadows produce simply an erection of the neighboring spines. Local illumination, however, evokes a somewhat different reaction. Throw a spot of light upon the surface of a sea urchin and the neighboring spines will bend over toward the illuminated spot, regardless of their previous position. The nearest spines are the first affected, but the reaction soon spreads, if the stimulus is sufficiently strong, to the outlying spines, so that one may observe quite a cluster of spines pointing toward the stimulated region. The reaction is practically the same as that given to a mechanical or chemical stimulus. After bending toward the stimulated point for a while the spines subsequently sway about in other directions and finally come to rest. The effect of a beam of light thrown upon an extended tube foot is to cause it to contract in an irregular manner. The tube feet do not seem to show any particular response to shadows.

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[2] Zeit. f. Biol., 40, 447. 1900.

The reactions of the spines to light are independent of the central nervous system. Pieces of shell from the inner side of which all traces of the radial nerves were scraped away, were exposed to a concentrated beam which had traversed several inches of water. The spines would still bend toward the illuminated spot and could be made to bend over in any desired direction by throwing the beam on different points. The response of the spines to light depends upon the superficial nervous plexus as Von Uexküll found to be the case for their reactions to mechanical stimuli. How far these individual reactions of the parts help us to explain the behavior of the animal as a whole is a subject reserved for later discussion.

EFFECT OF REMOVING THE SPINES

In several individuals the spines were entirely removed from the lower hemisphere of the test, leaving only spines which were incapable of touching the bottom. The specimens were then placed in a dish of sea water and exposed to the light. Soon they began slowly crawling away from the light and all assembled at the negative end of the dish. When the ends of the dish were reversed the specimens crawled away from the light as before and continued to do so in several other trials. The rate of movement was considerably slower than that of normal individuals.

Removal of the spines and tube feet from one lateral half of the body produced circus movements in most cases which brought the animal slowly away from the light in an irregular course.

EFFECT OF REMOVING THE TUBE FEET

Several times individuals were observed walking on the tips of their spines with the test raised about a centimeter from the bottom. Careful observation with a large hand lens, both through the glass bottom of the dish and in other cases through the sides when the specimen was placed near one edge, showed that none of the tube feet reached the glass. To make it perfectly certain that none of the feet escaped observation amid the mass of spines all the tube feet which bore discs were cut off with a pair of fine scissors. The specimens thus operated on crawled away from the light repeatedly. Their rate of movement was somewhat slower than that of normal sea urchins, but their course away from the light was apparently as direct.

EFFECT OF CUTTING THE NERVE RING

Specimens in which the nerve ring is destroyed still show the local responses to the illumination of particular parts of the body. The spines bend toward the illuminated spot and bristle up when a shadow is thrown upon them. If orientation is effected by the direct action of light on the parts of the body on which it impinges it is quite within the bounds of possibility that a sea urchin with the nerve ring destroyed would still be able to orient itself and crawl away from the light. The spines and tube feet on the two sides would be stimulated equally, those on the end away from the light would receive the least stimulus, while those toward the light would be affected most. A sea urchin with its independently responding motor elements deprived of the control of a common co-ordinating nerve tract is to a certain extent comparable to a cœnobic organism, such as Pandorina or Volvox, whose individual cells respond to light in such a way as to cause a phototactic movement of the whole colony. When we reduce the sea urchin to a system of independently reacting parts it is a matter of interest to find what happens to its phototaxis.

Several dozen specimens were accordingly operated on, in some of which the nerve ring was cut in several places; in others the internal organs were mainly removed, leaving intact, however, the radial nerves. The specimens subjected to the more severe operation lived for several days and were able to move their spines and tube feet about freely. They were exposed at several different times to ordinary light, artificial light, and direct sunlight, but not a single individual showed the least trace of negative phototaxis. Even after several hours exposure they remained in very nearly the same position in which they were at first placed.

In the specimens in which the nerve ring was cut great care was taken to produce the least amount of injury compatible with certainly cutting the nerve ring. A fine, sharp, bent scalpel was inserted into the mouth somewhat farther than the level of the nerve ring and a small slit was made in five places. The specimens were placed in running water and showed little effect of the shock of the operation. Some were experimented with soon after the nerve ring was cut; others were allowed to remain quiet for several days before being disturbed. As a rule the individuals operated on were in good condition and even the organs nearest the cuts showed little change in the character of their responses to local stimulation. Local responses to light and shadows occurred in the same manner as in normal sea urchins, and the general responsiveness of the animals indicated that shock effects played a comparatively small part in causing any changes which may have occurred in their behavior.

Numerous experiments were tried with the specimens operated on in order to give them every possible opportunity of showing a phototactic response. Neither in dim light, nor in direct sunlight was there any well defined movement away from the stimulus. Most of the experiments were carried on by exposing one-half of the animal to direct sunlight while the other half was shaded, as sea urchins have been found to respond most readily to light under these conditions. Continued experimentation with numerous individuals, with repeated intervals of rest in the dark to increase their sensitivity failed to elicit any movement away from the light in any sea urchin in which I was sure that the nerve cord had been cut. The lack of locomotor response was apparently not due to any failure in the response of the organs stimulated. If a sea urchin has one-half of the shell exposed and the other half shaded, the spines on the stimulated areas move more actively, as if they were making ineffectual attempts to carry the animal into the shade. The tube feet on the equatorial and upper regions of the body are more retracted on the stimulated side, while they are frequently much extended on the shaded side. The whole animal squirms and twists about uneasily, but it seldom gets more than a few millimeters from its original position even after hours of exposure, and is then as likely to become shifted in one direction as in another. Such a sea urchin gives one the impression of trying to get away from the stimulus, while being held back by the misguided action of some rebellious tube foot which fails to loosen its hold, or the contrary movement of certain spines. On carefully observing such an animal from below with a hand lens one can detect more or less working at cross purposes among the tube feet and spines. While the majority of the tube feet are very frequently directed away from the light there are several which do not fall into line with the others and whose failure to co-operate effectually checks the locomotion of the animal.

The direction in which the sea urchin moves is not determined simply by which motor elements exert themselves most vigorously. Locomotion is a corporate activity in which the various appendages co-operate much as they do in such animals as centipedes or insects. Cutting the nerve ring destroys the nervous connections through which co-ordinations are effected. The result is that there is so much misdirected activity that the sea urchin gets nowhere in particular. It may be that other kinds of sea urchins may exhibit a certain amount of phototaxis through the direct action of the parts most strongly stimulated by light. From our observations on the light reactions of the species experimented with such a result would not be surprising. Specimens operated on when first exposed to light would perform all the preliminary movements of the spines and tube feet as if they were going to crawl away like normal sea urchins; but not one of them got further than making a beginning.

GENERAL CONSIDERATIONS

We have seen that in the sea urchin light sets up a series of co-ordinated and definitely directed movements in relation to the source of the stimulus. It also sets up definite reflexes of particular organs of the sea urchin's body. The question naturally arises: How far can the light reactions of the animal as a whole be regarded as the result of these reflexes? Light falling upon the sides of the body causes the spines of those regions to make movements toward the light. We may assume that it tends to cause the tips to be depressed during their backward stroke, so that each spine performs a sort of rowing action which tends to push the body ahead, and there is a certain amount of evidence for this in the fact that the spines on the side toward the light are the most depressed while those on the opposite side are somewhat elevated. The spines on the posterior side of the body may be supposed to have their backward movement emphasized, while those in front, being less affected by the light, will act less vigorously or be somewhat raised because the light stimulates most the region lying above them. Light also tends to cause the tube feet to become more retracted on the stimulated side while they are extended freely in the shade. These reactions make for the locomotion of the animal away from the light, but by themselves they are insufficient to accomplish this result.

This circumstance does not I think afford a serious difficulty in interpreting the phototaxis of the sea urchin as belonging to the reflex type of behavior. Stimuli affecting one side of the body are responded to by movements of organs on the opposite side. It is probable that the sea urchin crawls away from the side stimulated by a beam of light for the same reason that it escapes from mechanical irritation. I have found that sea urchins in a dark room kept as free as possible from other sources of light except a small spot of intense light focussed on one side by a lens would crawl away from the source of stimulus. The light was passed through several inches of water to filter out the heat rays, and was screened away so that it had little chance to affect directly the parts which were active in carrying the sea urchin away. Since the animal had just shown itself irresponsive to light considerably stronger than that which reached any other than the spot most stimulated, we are justified in regarding its negative reaction as essentially like its crawling away from a point scratched by a needle. In these reactions the tube feet let go their hold near the stimulated spot, and those over the lower surface bend away from the stimulated side, extend, attach and pull the animal away. Along with this there is a parallel co-operative movement of the spines.

In these reactions there is no *direct* response to a stimulus on one side of the body by a reaction of organs on the opposite side as in the crossed reflexes of higher animals. The first response is near the stimulated point. From here the movement spreads until all the locomotor organs of the animal are involved. The stimulus to the activity of the distant organs is not so much the primary stimulus received from the outside as the stimulus afforded by the activities of the intermediate organs, as in the chain reflexes of higher forms. This spreading of a certain consensus of action, if I may use the expression, is the means by which the sea urchin reacts as a whole to outer stimulation. It is beautifully shown in the righting movements of starfishes where there may be at first antagonistic efforts of various arms, which are sooner or later overcome by a series of co-operative activities which extends from one attached arm until all the others take part in the unified impulse to turn the body over. It is also shown in the capture of food and its transfer to the mouth. The performance of a certain act by any of the numerous organs of the body of a sea urchin or starfish seems to stimulate neighboring organs to a like activity. Upon this characteristic many of the co-ordinations to be observed in echinoderm behavior apparently rest.

The foregoing experiments were carried on at the Bureau of Fisheries at Woods Hole, Mass., and I wish to thank Dr. F. B. Sumner and Mr. Pope for many courtesies received during my brief stay at that place.

SUMMARY

Arbacia punctulata generally reacts negatively to light of a great range of intensity, although it is occasionally positive in weak light.

After it has ceased to respond it may be made to resume phototactic movements by mechanical disturbance.

Movements away from light are generally effected by the combined action of the spines and tube feet, although phototaxis may occur when either one of these sets of organs has been removed.

Arbacia will move against the rays of light to get into a shaded region.

Arbacia responds to shadows by an erection of the spines and not necessarily by a movement of the spines toward a shaded area. The response occurs only a few times, but is resumed after a short period of rest. The reaction is dependent upon the radial nerves.

Local stimulation by light causes the spines to move toward the stimulated part. This reaction occurs in isolated pieces of the test which have no part of a radial nerve.

Strong light thrown upon the tube feet causes them to be withdrawn.

Cutting the oral nerve ring does not interfere with the local reflexes of the spines or tube feet, but it prevents the sea urchin from crawling away from the light. While reactions to local stimulation by light are of a character to aid in the phototactic response they cannot alone effect the locomotion of the animal. Phototaxis, like movements away from mechanical stimulation, depends upon a co-ordinated activity of various organs of locomotion.

A NOTE ON THE BEHAVIOR OF THE WHITE RAT

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One figure

In the brief note which follows, I wish to place on record an observation made on some white rats relative to the question of learning a problem by being " put through." The behavior in question derives its chief interest from its relation to facts and theories of animal behavior as presented by Thorndike[1] and by Cole.[2] For this reason it will be well to summarize the results of those writers.[3]

Thorndike describes his tests as follows: "A cat has been made to go into a box through the door, which is then closed. She pulls a loop and comes out and gets fish. She is made to go in by the door again, and again lets herself out. After this has happened enough times, the cat will of her own accord go into the box after eating the fish."[4] If, however, the animal is dropped through a hole in the top of the box, the same number of trials will not suffice to induce it to turn, after feeding, and re-enter the box. " The only difference between the first cat and the second cat is that the first cat, in the course of the experience, has the impulse to crawl through that door, while the second has not the impulse to crawl through the door or to drop through that hole. So, though you put the second cat on the box beside the hole, she doesn't try to get into the box through it. The impulse is the *sine qua non* of the association."[4] Three cats were made to enter the box. All turned and went in of their own accord by the 37th trial. Three were dropped through the hole in the top of the box. One was given 50 trials; one, 60; and one, 75. All failed to go in of their own initiation.

Cole repeated Thorndike's experiments on four raccoons. Two series of experiments were given. In the first, (Thorndike did not give a series corresponding to this one), the animal was lifted by the skin of the neck and dropped inside through the *door* of the cage. Cole found that between the 22nd and the 57th trials all the raccoons turned and re-entered the box of their own accord. From these tests, Cole concludes that any raccoon will re-enter the box at least by the 100th trial. In the second series, the animals were dropped through a hole in the top of the box as in Thorndike's experiments. By the 22nd trial all the raccoons used in this latter test climbed upon the box, *via* a step arranged for that purpose, and dropped inside. All, however, had been trained in the first experiment, i.e., in that of re-entering through the door.

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[1] Thorndike, E. L. Animal Intelligence. 1898. *Psych. Rev. Mon. Supp.*, vol. 2.

[2] Cole, L. W. Concerning the Intelligence of Raccoons. 1907. *Jour. of Comp. Neur. and Psych.*, vol. 17.

[3] Other students of animal behavior have worked with this problem of " putting through," but they have not used the particular method here under consideration.

[4] Op. cit., p. 67.

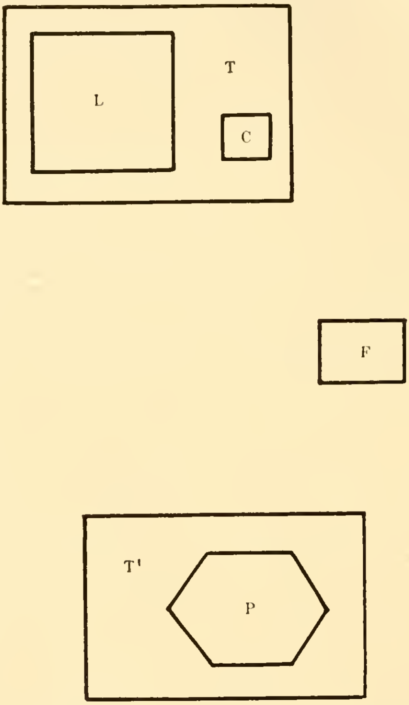

FIGURE 1. Plan of the arrangement of the boxes in the present tests.

Let us now turn to the observations on the white rat which it is the purpose of this paper to compare with the above results obtained with cats and raccoons. The rats in question were working on a problem in which the being "put through" was merely incidental. The apparatus was in the same room as the living cage, but on a separate table. Fig. 1 indicates the essential relations of the boxes to one another. *T* is the table upon which are placed the living cage, *L*, and a small accessory box, *C*. This latter is of wire and is 12 x 7 x 9 inches with an opening in the top 5 x 5 inches. *F* is a chair upon which food is placed during the day's experiments. *T′* is the table upon which the problem box, *P*, is placed. For 204 days, once each day, the rats were taken from *L* one by one and each given five trials in the problem box. The reward of each of the five trials was a bit of food given on the table *T′*. As the day's work with each rat was finished, the animal was picked up, by placing the hand about the body, and dropped into *C* through the hole in the top. When all the rats had been placed in *C*, a small quantity of food was put in the living cage, and the animals were picked up, lifted through the door of this cage and dropped within.

On the 204th day after the experiments were begun, the door of the living cage was accidentally left open, permitting the animals to come out upon the table. They ran about upon this, frequently pausing and sniffing the air in the direction of the food at *F* and of the experimenter who stood between the two tables. Within two or three minutes all the rats (five in number) had come to the box *C*. It was smelled of cursorily and passed by. But two of the rats climbed upon the box and dropped down inside through the hole in the top,—this despite the fact that they had never been fed in the box and that the rat odor alone could be there. This behavior they repeated on each of the next eight days and at frequent intervals thereafter until the close of the tests. The reason why the other rats did not eventually learn the reaction and why the two who had learned it tended to discontinue the reaction is undoubtedly to be found in the fact that the box *C* was not used in the experiments after the 204th day.

The complete method of handling the rats, when stated in terms of the symbols used in Fig. 1, is as follows: L-P-Food-C-L-Food. There are thus two complete cycles each culminating in the acquisition of food. When the rats were prevented from going through the first cycle, they initiated the second by climbing into *C*. This is not a case of abbreviated reaction similar to one noted by Cole.[5] There the raccoons shortened the path by which they climbed into the box: here the rats merely initiated the second complete cycle by necessarily eliminating the first one. In all essentials the behavior of the rats and the raccoons is in agreement. The reader may object, however, that the two sets of results differ fundamentally in that it is not known but what the rats would have behaved in the manner described even though they had not been "put through;" that, in other words, it was either chance or curiosity which led the rats into the box. In answer to this I submit the following: (1) The two rats that entered the box stayed in it until taken out. Had they been exploring merely, they would have left the box in a short time. (2) Chance is not the explanation, because the behavior was repeated for a little more than eight days. Again, (3) evidence against both chance and curiosity as the explaining factors is to be found in the fact that three rats failed to enter the box although the two others, as just stated, did so for some days. All five rats were close together on the table, so that this failure of three rats to follow the other two is very noteworthy in light of the results obtained by Berry.[6] This investigator records many instances where rats that had failed to leave a box, by doing which they would have secured food, did leave the box after they had seen (?) others give the reaction several times.

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[5] Op. cit., p. 239.

Let us now attempt a correlation of the three sets of data above given. Cole's results and those of the present writer oppose Thorndike's in the sense that both find that some animals (even those as low in the scale as the white rat), will learn certain problems by being put through them. Two possible reasons may be given to account for the fact that Thorndike's cats did not learn the reaction in question: First, they were not given enough trials. On *a priori* grounds, it is a valid assumption that it may take a much larger number of trials for an animal to learn a problem when being put through than when it experiences its own initiation of the act. Yet on the middle of page 67,[7] the inference is to be drawn that Thorndike thought *no* more trials should be required. Even twice the number of trials is not sufficient to justify the conclusion that an animal cannot learn by being put through.[8] A second and probably the fundamental reason why the cats did not learn the reaction is that dropping through a hole runs counter to native tendencies manifested in their behavior. Had Thorndike dropped the cats through the *door* as Cole did in one series of tests with his raccoons, it is not at all improbable that different results might have been obtained. One who has had occasion to observe the behavior of cats and raccoons can hardly have failed to notice their radical difference when it comes either to entering or to being put into a hole. Raccoons and rats, in their natural habitat, are accustomed to going in and out of holes in reaching and leaving their nests. In captivity they make no particular resistance when being put through a hole. Cats are just the reverse. They avoid dropping through holes, and it is all but impossible to put one through a hole. Thorndike has only recorded the vigorous resistance which his cats made to confinement. We are not told of their behavior when being put through the hole in the top of the box.

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[6] Berry, C. S. The Imitative Tendency of White Rats. 1906. *Jour. Comp. Neur. and Psychol.*, vol. 16.
See particularly experiments I and IV.

[7] Op. cit.

Thorndike used his results to prove that cats have no ideas to associate, only impulses. Cole, whose results are just the reverse, uses them in his conclusion that raccoons have mental images. If this interpretation be accepted as valid and Thorndike's experiments be regarded as conclusive, we are forced to take the position that rats, too, have mental images. Their mental life is thus on a higher plane than that of the cats. Few comparative psychologists, however, will consent to this relative classification of the animals.

If the various objections above noted be accepted, then the question of ideas is irrelevant to the cats' behavior in the present tests. It remains to be seen that this is also true for the behavior of the raccoons and rats. Reasons have just been given why instinctive tendencies may keep a cat out of the box, but it is doubtful whether instinctive tendencies will get raccoons and rats into boxes. The influence of instinct here is rather negative than positive. The effective factor is association. The constant use of the box as a link in the food getting series has made it attractive in itself, or, to state the matter differently, the very perception of the box has acquired motive power.

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[8] Op. cit., p. 66.

NOTES

THE DISCRIMINATION METHOD

The discrimination method, as described by me in the "Dancing Mouse"[1] and in greater detail in my report with Watson on "Methods of Studying Vision in Animals,"[2] demands, in the light of results which have thus far been obtained with it, certain criticisms. What I have to say at present in connection with the method is intended for the guidance of investigators who may wish to employ it in the study of sensory discrimination or other forms of organic reaction.

The method, as described in both references, consists, essentially, in the presentation to an animal of two objects which differ with respect to one or more characteristics on the basis of which the organism may select the one and reject the other. In one form, the method consists in the presentation of two visual areas of like size, form, and brightness whose discriminable difference is color. In another form, it consists in the presentation of visual areas which differ only in brightness, in size, or in form.

The conditions of discrimination, as thus presented to an animal, are somewhat unnatural, first, because the method is, as a rule, employed in a dark-room, and, second, because the objects to be discriminated differ in fewer respects than do objects which are usually dealt with in nature. Both of these possible objections to the method may be met satisfactorily by the experimenter, for the method may be employed advantageously under widely differing conditions of general illumination, and the objects to be discriminated may be varied in complexity as seems to the experimenter desirable.

In certain recent investigations with birds and mammals, the method has yielded negative results even after extremely long periods of training. I wish to point out that in some of these investigations the experimenters probably have wasted a considerable amount of time and energy by presenting to their animals visual objects which differ in only one respect: for example, in color, in brightness, in size, or in form.

It seems fairly certain that in our experiments we should not aim directly at our goal but, instead, begin with conditions which are rather more natural than those demanded for the accurate testing and measuring of any particular kind of visual discrimination by either the "brightness apparatus" or the "color apparatus." In an investigation of the perception of size, for example, it would seem safer to begin by using objects which differ in several respects. Thus the animal may more quickly be brought to attend to the proper objects and be trained to react appropriately.

Let us suppose that the initial condition of the experiment involved the use of objects which differed in brightness and form as well as in size. Then, just as soon as the appropriate form of response appeared, the experimenter would, by eliminating the three factors one after another, ascertain the following important facts: first, which of the three characteristics of the objects the animal naturally uses, as a basis for discrimination; second, the order of importance of the three factors for the purpose of discrimination; third, the ease, accuracy, and degree of delicacy of discrimination by means of each of the factors. Should the organism depend upon brightness at first, the experimenter might gradually diminish the brightness difference of the two objects until dependence upon it for discrimination became impossible. He might thus gradually bring the animal to attend to either the size or the form of the objects, or to both. Sbould it depend upon size, difference in form might immediately be eliminated and the experimenter would then be free to proceed with his special study of the characteristics of perception of size in the organism.

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[1] Yerkes, Robert M. The Dancing Mouse. New York: The Macmillan Company. 1907.

[2] Yerkes, Robert M. and Watson, John B. Methods of Studying Vision in Animals. *The Behavior Monographs.* 1911. vol. 1, no. 2.

In this experimental procedure there are, obviously enough, two important points: first, the use of conditions at the beginning of an investigation which are readily discriminated by the animal and which therefore permit of the acquisition of a habit quickly and without risk of discouragement; second, the directing of attention from the more readily perceived to the less readily perceived factors or characteristics of the objects through the gradual elimination of the more important factors; third, the simplification of conditions until, finally, discrimination is made to depend upon a single sensory factor.

By employing the discrimination method in some such fashion as has been suggested, the experimenter may not only obtain definite knowledge of the relative importance of different visual characteristics of objects, but he may also succeed in getting his animals to distinguish objects by means of a factor which otherwise could not be made use of. The secret of success is the gradual progress from the more important to the less important sensory factors of the situation. Several of us have made the grave mistake of demanding of our animals attention to objects which, although markedly unlike for us, were probably very much alike for them, or we have made the no less serious mistake of beginning with objects which differed in a number of important respects for the animals, and as soon as discrimination appeared we have eliminated factors and thus *abruptly* increased the difficultness of discrimination. Naturally, we have experienced difficulties in getting our animals to attend to the objects and also in preventing discouragement and refusal to attempt to discriminate.

There is further the important possibility of saving time and of obtaining positive results where they otherwise would not be likely to appear by the introduction of still other variations in the objects.

Professor Watson, in using the discrimination method for the study of color vision, has employed the device of a moving stimulus in contrast with a stationary stimulus. Thus, for example, a red stimulus area which is constant in position, size, shape, and brightness is presented beside a green stimulus area of like size, shape, and brightness, but appearing intermittently. At first, the intermittent stimulus may be interrupted at intervals of one second. This serves to attract the animal's attention to the visual areas. Gradually the rate of interruption of the one stimulus may be increased until flicker entirely disappears and the animal's attention may then continue to be directed to areas whose only difference is one of color.

A similar device has been used by Professor Casteel in his study of visual discrimination in turtles.[1] He placed white designs on pendulums and caused them to swing slowly back and forth in order that they might "attract and concentrate the attention of the animals."

Another important variation in the method is to present a single stimulus thus having only one object upon which attention is to be concentrated. The organism may be trained either to seek or to avoid the given object. Later, this object may be presented with a more or less similar object and discrimination on the basis of certain characteristics demanded.

Other variations may be introduced as seems desirable by the employment of stimuli for different senses. Visual and olfactory stimuli may be employed simultaneously, or olfactory and auditory, or visual, olfactory, and auditory. In certain cases, it may prove very advantageous to introduce stimuli which are naturally interesting to an organism even although those stimuli are not to be used otherwise than to control the attention and encourage the organism to effort.

Every experimental study of an organism emphasizes the importance of knowledge, insight, and ingenuity on the part of the investigator. He can not know too much about the habits, instincts, and environmental relations of the organism under consideration. Nor can he be too much given to distrusting and discounting the value of human experience as a guide in the study of animal behavior.

Finally, it may not be superfluous to emphasize the importance of avoiding the overworking of animal subjects, for especially in the higher organisms it is clear that too frequent necessity for reaction to a situation, even although the reaction can be made appropriately, has a bad effect upon the organism. It is extremely important, also, to avoid discouragement on the part of the animal. This may result from too frequent experimentation, too prolonged experimentation, or demands for discrimination under conditions which are overdifficult.

In connection with these dangers of experimentation, overfeeding is a frequent mistake on the part of experimenters. It is a good laboratory rule to supply animals with an abundance of sunlight, fresh air, to make them work for a living, and to restrict the amount of food obtained to that necessary to keep them strong and active.

ROBERT M. YERKES

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[1] Casteel, D. B. The discriminative ability of the painted turtle. *Journal of Animal Behavior,* 1911. vol. 1, p. 15.

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EXPERIMENTS ON THE BRIGHTNESS VALUE OF RED FOR THE LIGHT-ADAPTED EYE OF THE RABBIT

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I. METHOD

The most fundamental source of error in experiments on color discrimination in animals lies in the possibility that the animals tested, while they discriminate successfully between objects which appear to the human eye of different colors, are in reality seeing the colors as different shades of gray, which would be the case with a color-blind human being. It used to be considered a sufficient precaution against this possibility if the color used were matched with the gray that a color-blind human being would see in its place, and if it were then shown that the animal tested was able to distinguish between the color and its equivalent brightness to the human eye. The work of Yerkes on the dancing mouse [1] and that of Watson on the color-sense of the monkey [2] have made us realize, however, that the brightness value of a color to the eye of an animal may possibly be quite different from its brightness value to the human eye, so that the animal may successfully discriminate between a color and its equivalent gray to the human eye and still see the color merely as a gray. For the same reason, it is no longer sufficient proof of color vision to show that an animal's choice of a given color is unaffected by such wide variations in its saturation as would to the human color-blind subject cause great differences in the grays seen: though a very dark red and a very light red are equally chosen when presented with a medium green, it is possible that to the eye of the animal even the light red may look darker than the green, so that the discrimination may be based on brightness alone. Evidently, an important step in the investigation of color vision in an animal is to find the brightness equivalent of a given color to the eye of that animal. If it proves to be the case that the animal tested can learn to discriminate a color, not merely from the gray of equal brightness to the human eye, but from every one of a series of brightnesses ranging from black to white, then we shall have ground for inferring that the color is indeed seen as a color and not as a gray.

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[1] Yerkes, R. M. The Dancing Mouse. New York. 1907.

[2] Watson, J. B. Some Experiments Bearing Upon Color Vision in Monkeys. *Jour. Comp. Neur. and Psych.*, vol. 19, pp. 1-28. 1909.

Our problem in the present research was to find, if possible, the equivalent brightness for red to the light-adapted eye of the rabbit. We chose red, because the results of Yerkes and Watson have indicated that the brightness value of this color is probably different and markedly less for the eye of the dancing mouse and the monkey than for the eye of man. We also made some series of experiments to investigate the brightness value of blue, with less conclusive results.

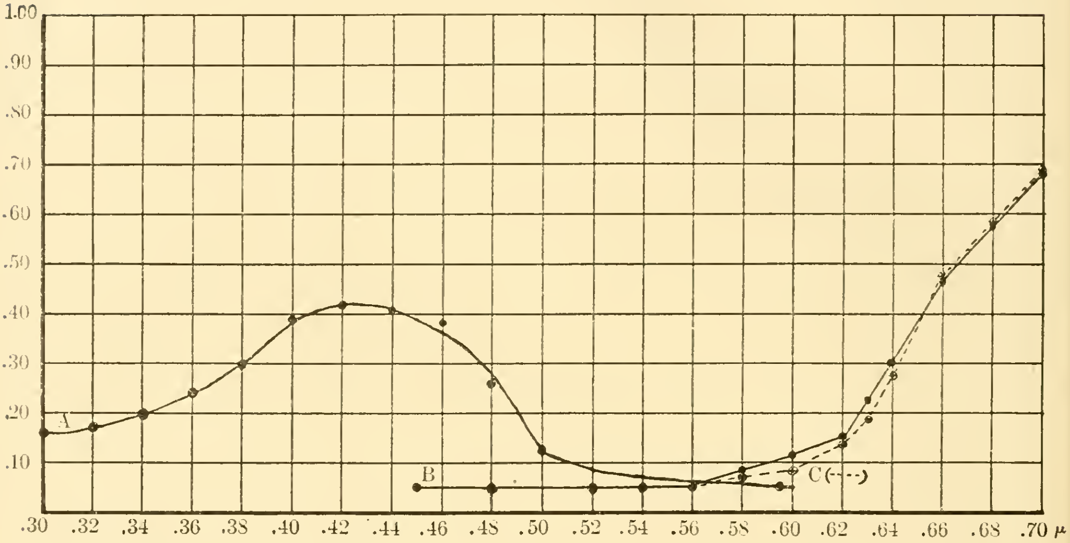

Figure 1. Color values of Bradley papers. Curve A, blue; Curve B, red; Curve C, "darkened" red.

Our work was done with the Bradley colored papers and the Hering gray papers: we also used a "black" paper supplied by the C. H. Stoelting Company, of a brightness more nearly equivalent to that of Hering gray number 46 than to any of the other Hering papers. While it is desirable that all final work on color discrimination in animals be done with pure spectral colors, yet as the apparatus for the use of such colors as stimuli is complicated and costly, it is, as Yerkes and Watson themselves admit,[3] worth while to attempt preliminary work under simpler conditions. Certain sources of error, other than the impurity of the colors, pointed out by Watson as involved in the employment of colored papers, we have, as will be indicated, avoided by slight modifications of our method. Figure 1 shows the reflective power of the colored papers used, measured by means of a MgO screen. Curve A indicates that of the Bradley saturated blue paper; curve B that of the fresh Bradley saturated red paper, and curve C that of the Bradley saturated red paper somewhat darkened by time. The tests of the red paper were made through the kindness of Professor Herbert E. Ives of Cleveland.

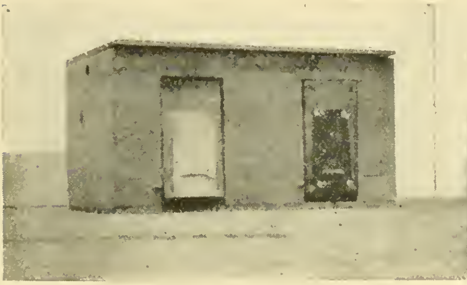

FIGURE 2. Color vision apparatus for testing rabbit. Doors closed.

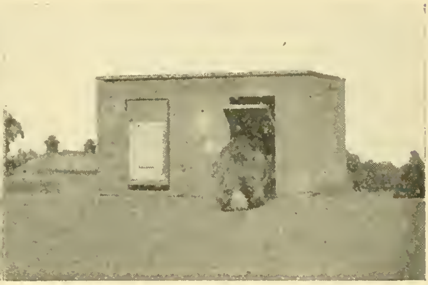

FIGURE 3. Same, showing rabbit opening door.

Our apparatus consisted of a wooden box, 24 inches long by 13 inches deep and 13 inches high, divided into two equal compartments by an inside partition. Each of these two compartments opened by a door pivoted so as to swing on a horizontal axis; the two doors appeared side by side when the box was viewed from the front. They were 9 inches high by $4\frac{3}{4}$ inches wide. Each door could be fastened on the inside by a button. The lid of the box was hinged so that food could be put in and the doors fastened and unfastened between experiments. On one of the doors was tacked with thumb-tacks a piece of Bradley saturated red paper, somewhat smaller than the door; to the other door was tacked a piece of Hering gray paper of the same size and shape as the red paper. Food was placed in both compartments of the box; but the door carrying the gray paper was fastened on the inside. A rabbit approaching the red door and pushing the lower part of it could get food from the inside; the gray door would not open. The places of the red and gray papers were exchanged in successive experiments (see figures 2 and 3).

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[3] Yerkes, R. M., and Watson, J. B. Methods of Studying Vision in Animals. *Behavior Monographs*, vol. I, no. 2. 1911.

One of Watson's objections to the use of colored papers is that if pasted on supports they cannot fail to present inequalities of surface, wrinkles, which will make them easily distinguishable by this means alone. Evidently here, where the papers were not pasted but pinned on the doors in each succeeding experiment, the irregularities of surface could not possibly serve as means of identification, since they were probably different each time the papers were fastened on the doors. Another possibility of error pointed out by Watson as involved in the use of colored papers is that papers of different colors may have differences of surface texture that would render them discriminable even by a color-blind subject. As will be seen, our rabbits proved able to learn the discrimination between red and various grays, but unable to acquire that between red and the black papers used: yet the surface texture of the red paper to ordinary human vision differed more from that of the black papers used than it did from that of the gray papers. A special test of the "texture" possibility of error was made with two of our rabbits. One of them, "Abednego," had made a perfect record in discriminating red and gray number 15 for fifteen consecutive days, eight tests being performed a day. On the sixteenth day pieces of red and grey velvet, matching the papers as closely as possible, were substituted for them. The character of the surfaces here should have been the same, and should have been sufficiently unlike that of the paper surfaces to confuse the rabbit if he had been depending on surface texture. He made a perfect record in the ten tests made, although he seemed much puzzled by the unaccustomed "feel" of the velvet on his nose, and twice *after* touching the red drew back and seemed to look at it. The other rabbit, Polly, gave exactly similar results: an interpolated series of ten velvet experiments did not cause her to make any errors in the discrimination of red and gray number 15.

The *smell* error in its various forms was dealt with as follows. Since food was always, and in equal amounts, in both compartments, the animals could not have been guided by the smell of the food in the box. If the same door were open in two successive experiments, the rabbit might be guided by the smell of food on the ground in front of the door, since the food was usually pulled out to be eaten; or by the smell left from previous nosing of the edges of the door. Throughout the greater part of the experiments, however, the open door was on alternate sides in succeeding experiments. Thus the strongest smell of food on the ground or of nosing on the door was regularly on the wrong side; that is, the side that had been the correct side in the preceding experiment. Further, when a given discrimination began to be learned, the correct paper was naturally pushed oftener than the wrong paper; and if the same pieces of paper had been constantly used and shifted from one door to the other, of course the correct piece of paper, which was the red in the experiments with red and greys, would accumulate more of the odor of being nosed than the wrong piece would. Hence we were careful to use a fresh piece of red paper in each test. After the lapse of a few days we used again papers that had been previously employed; from time to time entirely fresh papers were cut, and as we never noticed that the use of perfectly fresh correct papers disturbed the rabbits in the least, we concluded that the odor left by nosing on a paper was not influencing their choice. Again, it is quite conceivable that to a rabbit the different papers used had different intrinsic odors. That this was not a factor of any importance in determining the behavior of our subjects we demonstrated in the following way. After two of the rabbits (Light Nose and Dark Nose) had been in training for twenty-eight days, four tests being given each rabbit a day, a horizontal slit about 5 mm. wide and 10 cm. long was cut in both the red and grey papers at about the level where the rabbit's nose touched them in pushing the doors. Under this slit in the red paper was placed a piece of the grey paper, and *vice versa*. If the animals had been guided by the differing smells of red and grey paper, they must have been somewhat disturbed by an arrangement which brought the grey paper smell in contact with their noses on the red door. The arrangement made no striking change in the appearance of the two doors. Neither of the rabbits was at all influenced by the new state of affairs: both made equally good records before and after the change. This arrangement of the papers was used in all subsequent experiments with these rabbits: it was used from the beginning in the experiments on three other rabbits (Shadrach, Meshach, and Abednego); and was introduced in a few series with the remaining rabbit (Polly) without producing any change in the percentage of correct choices.

As a routine procedure in all the experiments except the very first ones, where we were habituating the rabbits to the apparatus, we kept out of sight of the animals tested.

It has been stated that our ordinary practice was to have the door which could be opened on opposite sides in successive experiments. It is therefore possible that our rabbits, instead of acquiring a habit based on discrimination between two visual stimuli, were, as their records improved, merely acquiring a habit based on kinaesthetic data, of turning to alternate sides in successive experiments. It may naturally be asked why, in view of this possibility, we did not vary the place of the open door less regularly. Our reason for not doing so at the outset lay primarily in the very strong tendency of the rabbits to form place associations, and to go to the side at which they had last received food. If they were allowed to get food from the same side twice in succession, a tendency to go to this side would be started, and we wished, of course, to force them to depend on visual clues from the first. In our earlier series we were testing two rabbits alternately, and there seemed little antecedent probability that with the comparatively long interval between successive tests with the same animal which this involved, a habit of alternating could be formed. Our later experience showed us, as will appear, that even when the tests followed each other immediately no habit of alternating was formed. The tendency was always rather to go to the side that had been found profitable the last time, and the only exceptions to our practice of alternating the positions of the open door occurred when, to break up a habit of going always to the same side, the open door was kept persistently on the opposite side.

The *motive* leading the animals to push the door, and to acquire an association with one visual impression rather than with the other, was simply the desire of food. The degree of hunger was certainly not constant. The tests were made almost without exception early in the afternoon, between half past one and half past three. At about nine in the morning the rabbits were given a breakfast of oats. The food used in the tests was always some form of green food, carrots, lettuce, apples, and in season, clover. After the tests were over for the day the rabbits were allowed to eat all the green food they wished. Usually, they seemed to have excellent appetites for the food in the experiment box, but there were variations, and since we had no means of controlling and estimating the strength of our motive, we do not wish to draw any conclusions regarding the speed of the learning process involved. In case of a wrong choice, the only form of punishment involved was sometimes merely the experience of pushing at a door that did not open. More often, the experiment box was removed before the rabbit had had time to go to the other door. But when a new discrimination was being learned, for a short time the rabbit was allowed, after pushing in vain at the wrong door, to go to the correct door and get food. This was occasionally allowed, also, when an animal had made a number of wrong choices in succession and seemed in danger of becoming discouraged.

Another factor which varied in different series, and whose variations would prevent our drawing inferences regarding the speed of the learning process, was the number of tests made on a single animal in a day. In the earlier series only four were made; in some of the later ones eight, ten, or twelve.

A choice was always counted as wrong if the rabbit went near enough to the wrong door to touch it with the long whiskers; for even the lightest touch on a fastened and wholly unyielding door felt quite different from a touch on an unfastened door.

Until the weather became very cold, the two rabbits tested in the autumn of 1910 were kept in a hutch and small yard built for them on the laboratory roof, and the experiment box was placed in this yard for the tests. All the tests with the rabbit used in the fall of 1911 (Polly) were made in this manner. Later in the winter of 1910-1911 the rabbits, except Light Nose, were kept and tested in one of the small rooms of the laboratory, which was brightly lighted by a skylight and a north window. For several hours a day during the winter they were allowed the run of the large laboratory room. During the period when the first series were being made, in the yard attached to the hutch, the two rabbits used as subjects (Light Nose and Dark

Nose) were tested alternately, four tests being made with each on a single day. The rabbit to be tested was admitted to the yard from the hutch door, The experiment box was placed opposite the hutch door, about five feet away from it. As the hutch door was nearer the left side than the middle of the hutch, it was impossible to place the experiment box in such a way that the rabbit should not enter the yard more nearly opposite the left than the right hand door of the experiment box. This fact seemed to have no effect, however, in producing a tendency to push the left hand door oftener than the right hand one. After one rabbit had finished a test, it was driven back into the hutch, and the gravel on the floor of the yard was raked over, to prevent the second rabbit's being guided by the smell of food on the ground where the first rabbit had been eating: the second rabbit was then admitted to the yard for a test. The series made with Polly in the autumn of 1911 were carried on in the same way, except that no other rabbit was being tested at the same time. In the experiments performed in the laboratory room, the rabbit to be tested was taken alone into the room and dropped about three feet in front of the experiment box, which stood on the floor. This method of confronting the animal with the box is not in general to be recommended, as unconsciously the experimenter might give the rabbit an impulse toward one or the other door. But since the results of these series confirmed those obtained under the other circumstances, and since under both conditions the discrimination was acquired in the case of red and grey and not acquired in the case of red and black, no such error seems to have been involved.

II. SUBJECTS.

Our first subjects were two young brown rabbits, a male, Light Nose, and a female, Dark Nose, obtained from a dealer in animals and probably born in captivity, though upon this point the dealer could not enlighten us. Nor did we know their age, but they were neither full grown nor sexually mature. We began our work with these on November 18, 1910. The experiments with the female continued until June 12, 1911, with three interruptions, one of eighteen days during the Christmas recess, one of twelve days due to illness on the animal's part, and one of four days during the spring recess. With these exceptions she was tested every day. Work with the male lasted until March 12, 1911, at which time we sent him away: he was tested, or at least the attempt was made to test him, every day with the exception of the interval during the Christmas holidays. On January 22 a litter of three males was born (Shadrach, Meshach, and Abednego). Tests on these were begun when they were 24 days old, and were continued with Shadrach and Abednego during the greater part of the time until June 12, 1911. The entire family was disposed of at this date in June. The next fall we obtained from the same dealer a young female (Polly) whose exact age and antecedents were also unknown, and experimented with her from October 3 until December 21, 1911. With the exception of the illness of Dark Nose, above mentioned, all the rabbits remained on the whole in excellent health and spirits during the whole period of work.

As was to be expected, the individual characteristics of the six rabbits differed considerably. Light Nose, the father of the litter was nervous, quick at learning, but decidedly unstable in temperament. After making a perfect record on one day, he would on the next day pay little attention to the box, but frisk about or push the wrong door apparently from a mere vagrant impulse. Dark Nose, the mother, was more stolid and more steady: it was long before she succeeded in making a perfect record, but when a discrimination was once acquired, she maintained a continuous high level of accuracy which her mate never acquired. She showed less tendency than any of the other rabbits to be distracted from the problem in hand, and in working on the difficult discriminations, while she would shake her ears over a mistake, she showed no tendency to delay subsequent trials because of previous failures, but worked with mechanical regularity throughout. The maturing of the sexual instinct in the male put an end to satisfactory work with him. If he was allowed to be in the hutch with his mate at the time of the tests his interest in her overcame his interest in food, and when he was separated from her he was often too restless and ill-tempered to test. For this reason, and because we did not have time to test five rabbits thoroughly each day, we discontinued work with Light Nose in the spring.

Of the three young rabbits, Abednego was most thoroughly tested. He was docile and steady. Shadrach was the most sluggish and the tamest of the trio. Meshach was an animal with pronounced individuality. He was from birth smaller than the others, and very nervous; until he was about three months old he trembled violently whenever he was picked up. He was also more inclined to show anger by biting, or by thumping with his hind legs on the floor. On one occasion he was accidentally dropped, whereupon he had a kind of convulsion and stiffened out as if dead; he soon recovered, however. When the young rabbits were about two weeks old they were kept in a packing box, the sides of which were fully two feet high. The room in which the box stood was cold, especially at night. One morning Meshach was missing, and was found to have climbed out of the box—a feat which the others did not accomplish for more than a week later,—made his way across the room to the radiator, and established himself in the warmest corner behind it. The most striking instance of the unusual temperament of this small rabbit is found in the behavior which is responsible for the fact that he does not shine in the records. As will be seen, he learned like the others to discriminate red from grey. The other rabbits tested with red and black had found it impossible to distinguish them. As the term drew near a close in the spring, we decided to select one of the rabbits and train it continuously with red and black until the end, so that we might be sure of having given sufficient opportunity for acquiring the discrimination if it could be done. The choice fell upon Meshach. For the first three days he happened to make six or seven right choices out of ten: then came two days in which he went wrong as often as right; and on the next day after this, on being put down before the experiment box, he immediately ran away. Nothing would induce him any longer to push the doors. In the many subsequent trials on that day and on succeeding days, he would thump on the ground with his hind legs, make the peculiar shrill grunt which seems to be the only vocal expression of emotion a rabbit has, and run away on being confronted with the box. His appetite for the food that was given him was as keen as ever, but he never again pushed a door of the box. It seemed odd that a well-formed habit should be so quickly and finally inhibited by an experience of no more poignant unpleasantness than merely pushing at a door which did not open. No pain was involved, for the rabbits seldom pushed hard at a door which did not at once yield. These tests with red and black had been preceded by seventy experiments with red and grey number 15, in which Meshach had made only eight errors, reacting promptly and readily. While there were other cases where the rabbits took to reacting reluctantly and irregularly, Meshach was the only one who once and for all forsook the habit of opening doors.

Polly, the rabbit tested alone in the fall of 1911, had certain tricks of behavior which distinguished her. She used her fore paws much more than the other rabbits, and if a hand was held out to her would rush at it, grunting, with her mouth open, and paw it violently with her fore paws. She had perhaps been teased before she came into our possession by having food held out to her and withdrawn. She had much less tendency to gnaw objects than the other rabbits had.

Certain peculiarities of behavior were common to all the individuals tested. They all expressed *irritation* by stamping with the hind legs, and when much excited, by a shrill grunting, not very loud, however. The first time we heard this sound it was made by Dark Nose when pursued by her mate. The latter would sometimes make it when he was 'cornered' after he had been running at liberty: sometimes also when he was put down in front of the experiment box after he had attempted to run away. We never heard the rabbits make any sounds to each other, with the exception of Dark Nose's grunts when running away from her mate. There was never the slightest symptom of hostile behavior on the part of the young rabbits to one another: they showed no resentment whatever when their food was stolen from them. The father was never allowed to come near the young rabbits. Expressions of irritation seemed to have no social significance; they were made rather at situations than at other animals. In Dark Nose especially, whose training with the boxes was longest continued, we noted a comical shaking of the ears when she made a mistake. This movement was made occasionally by all the rabbits when they were irritated, as for instance when we were trying to catch them. Very likely its significance as a serviceable associated habit is that it frees the long ears from anything that may be caught upon them. The rabbits occasionally bit our fingers gently when we were holding them; we never saw them bite one another. The young ones were not sexually mature at the time our work with them ceased: since they were all males it is possible that they might have become more quarrelsome at maturity.

No sounds or movements indicative of *pleasure* were observed, with one exception. Scratching a point on the side of the jaw, under the ear, caused the ear on the same side to droop forward. This movement seemed like a reflex produced by touching a particular spot, yet there were times when it could not be elicited. The rabbits would sit still and allow their noses to be stroked and their ears gently pulled. They never sought these attentions, however.

Little *maternal behavior* was observed on the part of the mother rabbit. She made the usual nest in preparation for the arrival of her offspring, lining a deep hollow in straw with fur from her body. After the young were born we never saw her go near the nest. She manifested no reaction whatever when we took the young ones out, and if we brought her to the neighborhood of the nest she made every effort to get away from it. Probably this behavior is due to a protective instinct to avoid betraying the neighborhood of the nest. When the young ones had emerged from the fur covered nest and were scrambling about, the mother never nosed them, washed them, or paid the slightest attention to them in our presence; they seemed to offer no stimulus to her whatever. Twice we saw a young rabbit come up to her, throw itself on its back and begin to nurse, but on both occasions she moved off at once leaving it sprawling. At no other time did we see her feed them. Despite maternal indifference the litter flourished.

On the whole the rabbits seemed to have little *social behavior*. We never observed them playing together. They would run, jump in the air, and gnaw the floor and woodwork, but they did not chase each other or lie in ambush and startle each other as kittens do: this type of play is evidently characteristic of a hunting rather than a hunted animal. If one rabbit happened to come upon another suddenly it would jump back, but the play would go no farther. They often lay down in little groups, however, and one rabbit would come up to another and offer its face to be washed. The effect was amusing when two of them washed each other's face simultaneously.

This mutual washing seemed their most marked reaction to each other. Corresponding to their lack of social behavior with their own kind was the absence of any such recognition of human beings as a dog or cat would show. Although they were kindly treated and petted, they never seemed to notice us in any way, except that they invariably ran away when approached: no experience could overcome the instinct for flight, probably the strongest instinct a rabbit has. Very likely their lack of eye movements and the fact that, as we shall see, they probably use monocular more than binocular vision, contributed to produce this effect of ignoring us, since it is difficult to judge from its apparent gaze where a rabbit's attention is directed. We never got them to respond in any way to the sound of our voices. They would start at a loud noise, but ordinary noises left them quite unaffected; probably from a protective instinct.

Gnawing, digging in the dirt, running, leaping with all four feet in the air, seemed to be their chief activiites. When pursued they almost never ran in a straight line, but displayed the useful instinct of doubling. One instance of an individually acquired habit is interesting. In the small room where the experiments were carried on after cold weather set in there was a box containing earth. Dark Nose happened to jump in this box one day when we were trying to catch her, and allowed herself to be picked up from it and dropped before the experiment box. From that time on, with occasional exceptions, she made a practice of jumping into the earth box between experiments and waiting to be picked up from it and dropped in front of the experiment box. As soon as she had opened the correct door and got food, the experiment box would be removed, whereupon she would immediately run to the dirt box, jump in, and wait, often for several minutes, until the experiment box was ready for the next test, when she would allow herself to be picked up and put before it. Another box containing earth stood in the large laboratory room. When all the rabbits had been tested it was our custom to catch them and put them into the hutch on the roof to spend the night. After the female had formed the habit of jumping into the earth box in the experiment room, she would ordinarily submit to being caught in the large room only when she had jumped into the earth box there.

Her trick of getting into the box and waiting until we were ready to test her proved so saving of our time that we dropped the small rabbits into the same box for safe keeping between experiments with them. After two days of this Abednego formed the habit of jumping in himself, and the other young ones acquired it shortly after. This behavior is interesting when one recalls that Thorndike's cats which had been picked up and dropped into a box never formed the habit of going in themselves[4]: precisely such a connection between being dropped in and jumping in was readily formed by our young rabbits. They would display amusing eagerness for the next test to begin after having jumped into the box, standing on their hind legs and peering over its sides.

It is of course probable that the peculiarities of instinctive behavior which we have described were influenced in a greater or less degree by the domestication of the animals.

III. RESULTS

The plan of our experiments, so far as the arrangement of series is concerned, underwent various modifications in its progress, and therefore will not make upon the reader the impression of ideal order and system. If we could have foreseen these modifications from the outset, there would have been the less need of making the research at all. We shall treat separately the results obtained from each rabbit.

A. *Experiments with Light Nose* (*male*)

Series 1. Red and Hering grey number 15. Red opens. All of the experiments of this series were performed in the yard attached to the hutch, Light Nose and Dark Nose being tested alternately, as described on page 150. Four tests were made on each day. The rabbits took very kindly to opening the doors, needing only to be shown once or twice that there was food in the box. The *total number of experiments* was 71. The *total percentage of correct choices* made by Light Nose was 63. Calculated at intervals of three days, that is, for every twelve tests, his percentages were 77, 64, 43, 84, 50, 43. It will be seen that his work was irregular, apparently because of playfulness. The series lasted from November 29 to December 16, 1910.

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[4] Thorndike, E. L. Animal Intelligence. New York. 1911. Pp. 101–2.

Series 2. Red and Hering grey number 7. This was the lightest grey we used in connection with red. Red opens. This series was performed under the same conditions as the preceding one. The *total number of experiments* was 30. The *total percentage of correct choices* made by Light Nose was 84. His percentages calculated for the first three days was 62; for the second three days, 95; for the third two days, 100. The series lasted from January 4, 1911, to January 12.

Series 3. Red and Hering grey number 24 (a darker grey than the others). The conditions were as before. The *total number of experiments* was 73. Light Nose made a *total percentage of right choices* equal to 65. His percentages calculated every three days were 75, 50, 75, 75, 58, 46. His falling off at the end was due to the fact that the presence of his mate was becoming too much of a distraction to him. From this time on we were obliged to separate them. The series lasted from January 13 to January 31, 1911.

Series 4. Red and Stoelting black paper. Red opens. The paper thus designated is that supplied in rolls as black paper by the C. H. Stoelting Company. It varies somewhat in brightness, but stands closer to Hering grey number 46 than to any other of the Hering greys. Light Nose's mate was removed from the hutch and yard during these experiments, and Light Nose was tested alone in the yard. His four tests therefore followed each other at a shorter interval than had previously been the case; and if his previous successes in choosing the red door had been due to a habit of alternating from side to side, this habit should now have had more favorable opportunity to develop. There were 32 experiments in the series. The *total percentage of correct choices* made by Light Nose was 37. The reason why it dropped below 50 was that the rabbit quickly formed the habit of going to the right-hand door in nearly every test. We occasionally tried to break up this habit by putting the red paper on the left-hand door in several successive tests, instead of alternating it from side to side. Fourteen tests were made with the red on the right, in eleven of which the rabbit chose correctly; and eighteen with the red on the left, in which he made only one correct choice. This formation of a place association under conditions which should have strengthened an alternating habit, had one existed, but which substituted a much darker grey for those previously used, is strong evidence that the rabbits were depending on a visual clue. The series lasted from February 13 to February 20.

Series 5. Red and Hering grey number 7. Red opens. The object of making this series of experiments with the same grey used in Series 2 was to find whether the failure of Light Nose with the red and Stoelting black was due to the difficulty of the discrimination or to some other cause, such as the separation of the rabbits or their physical state. The conditions were as before. Light Nose had by this time, however, become very indifferent to the experiment box. On being admitted to the yard he would commonly sit down and begin a prolonged toilet, at the end of which he would perhaps stroll about the box. When he pushed a door it seemed more the result of a chance impulse than of one based on discrimination. The *total number of tests* was 26; his *total percentage of correct choices* was 65. No place tendency appeared. On the whole, despite the unsatisfactory character of his behavior, some explanation other than indifference seems required to explain the results of Series 4. Series 5 lasted from February 21 to March 1.

Series 6. Red and Hering grey number 15. The conditions were as before. The *total number of experiments* was 29, and the *total percentage of correct choices* was 70. These were the last experiments made with this rabbit, as the work with him consumed too much time. The series lasted from March 2 to March 11.

It will be seen that his percentage of right choices was above 50 in every case except the series where the Stoelting black paper was used.

B. *Experiments with Dark Nose (female)*

Series 7. Red and Hering grey number 15. Red opens. This series was made with the conditions as described under Series 1, and during the same period. The *total number of experiments* was 71, and the *total percentage of correct choices* made by Dark Nose was 70. Calculated at intervals of three days, that is, for every twelve tests, her percentages were 55, 60, 85, 61, 78, 60.

Series 8. Red and Hering grey number 7. Red opens. Conditions and time as in Series 2. The *total number of experiments* was 30. The *total percentage of correct choices* made by Dark Nose was 70: her percentage for the first three days was 80, for the second three days 75, and for the remaining two days 66.

Series 9. Red and Hering grey number 24. Red opens. Conditions and time as in Series 3. The *total number of experiments* was 73. The *total percentage of correct choices* made by Dark Nose was 64. Her percentages calculated every three days were 66, 50, 75, 58, 66, 69.

Series 10. Red and Stoelting black paper. Red opens. In the experiments of this series Dark Nose was tested in the small laboratory room, being picked up and dropped in front of the middle of the experiment box, about three feet away from it. Her four tests a day followed each other at a shorter interval than before, when Light Nose was being tested in alternation with her. The same considerations apply with regard to the acquisition of an alternating habit as were noted under Series 4. The *total number of tests* was 58. The *total percentage of correct choices* made by Dark Nose was 31. Like Light Nose, she formed the habit of going to the right-hand door, and we tried to break up this habit by putting the red paper on the left-hand door a number of times in succession. In all, 35 experiments were made with the red on the left hand: in these she chose the red only once. We made 23 tests with the red on the right hand, and in these she chose the red 18 times. The formation of a place association by both Light Nose and Dark Nose when this black paper was substituted for grey is the more noteworthy since the external conditions were now somewhat different for the two rabbits. The series had the same general time limits as Series 4.

Series 11. Red and Hering grey number 7. Red opens. The conditions were as in Series 10. There were 61 tests in the series. Dark Nose made a *total of* 80 *per cent. of right choices.* In the first half of this series the right-hand tendency persisted: all of the choices were correct when the red was on the right, but only 36 *per cent.* were correct when the red was on the left. In the second half of the series her record was without an error. The series was made between February 21 and March 1.

On March 1 a series of six tests was given in which Stoelting black paper was substituted for red. The black was chosen each time.

Series 12. Red and Hering grey number 15. Red opens. This was the same combination as that used in the first experiments with Dark Nose and Light Nose. The conditions were as in Series 11. A *total* of 36 tests was made in the series, and Dark Nose made but one error, a record of 97 *per cent. correct.*

Series 13. Red and Hering grey number 15, with interpolated red and velvet black tests. On March 11 and 12, after her prolonged and almost errorless record with red and grey number 15, we gave Dark Nose four tests in which for the grey there was substituted the velvety black paper supplied with the Hering color-mixing disks. We were much interested to see her choose the black in all the four trials, wholly neglecting the red which she had been choosing with machine-like regularity. Finding the black door closed, she was allowed to get food from the red door. On the next day four similar trials were given her. The records for this day are in full as follows.

"1. Red on left. Sat still a long time, then went towards red, but swerved and went to black. Box removed.

"2. Red on right. Sat still. Went straight to black, pushed once and ran to red.

"3. Red on left. Very eager to be picked up and put before box. Sat still and seemed to look. Then went straight to red and ate.

"4. Red on right. Sat still a long time. Then went to red door, with a turn of the head towards black, and got food."

On the next five days we proceeded in the following manner. We gave on each day four preliminary tests with red and grey number 15. These were invariably correct choices, without any hesitation on the rabbit's part. Then two tests were given with red and velvet black, and then two final tests with red and grey. These final tests were also always without error. In all there were ten interpolated red and black tests. The results of these may be given in detail from the records.

"1. Red on left. Sat a long time, apparently looking intently, with nose working. Finally started with a jerk, went to black, barely touching it with a whisker. Box removed.

"2. Red on right. Went casually to left side of box, by black; touched black. Box removed.

"3. (Second day). Red on left. Hesitated; went to black. Box removed.

" 4. Red on right. Went straight to black. Box removed.

" 5. (Third day). Red on left. Went to black hesitatingly, barely touched it; box removed.

" 6. Red on right. Went hesitatingly to red.

" 7. (Fourth day). Went towards black, seemed to look at red, touched black. Box removed[5].

" 8. Red on right. Sat a long time facing the box. Finally sauntered to black side and casually touched black. Box removed.

" 9. (Fifth day.) Red on left. Touches black with nose. Box removed.

" 10. Red on right. Went straight to red."

Only twice in the ten interpolated red and velvet black tests did the rabbit touch the red, whereas in all the 30 red and grey experiments of the series she went at once to the red. A possible explanation for this may have been that the velvet black looked darker than the red to the rabbit, and that she had acquired the habit of reacting to the darker impression as such, that is, irrespective of the absolute brightnesses involved. This possibility will be considered later. Or, in view of the small number of the red and black tests, the choices of the black may have been accidental.

Series 14. Bradley saturated blue and Hering grey number 7. Blue opens. On the first day 12 experiments were made; on the succeeding days eight each. Otherwise the conditions were as in the immediately preceding series with this rabbit. The *total number of experiments* was 51: the *total percentage of correct choices* was 57. Her percentages for the successive days were 54, 62, 62, 62, 54, 50, 37. On the first day every choice but one was of the left-hand door. On the sixth day every choice but one was of the right-hand door. Various causes might have produced this poor record. The blue might have looked so much like the grey that the rabbit found it difficult to distinguish them; or, if she were capable of acting on the principle of choosing the darker impression as such, and found the blue lighter than the grey, she might have been unable to reverse her habit of choosing the darker; or, finally, the blue might have made upon her a qualitatively new (color?) impression, which she did not have time to learn to associate with food. The one hypothesis made unlikely by these results is that in the preceding experiments with red and grey she had been avoiding grey as such rather than choosing red, for continuing to avoid the grey would have produced a perfect record in this series. The series lasted from March 18 to March 24.

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[5] See page 151.

Series 15. Blue and Hering grey number 15. Blue opens. The experiments of this series were made at the rate of eight a day. There were 56 in all, and the *total percentage of right choices* was exactly 50. The percentages for each day were 62, 62, 50, 50, 25, 37, 62. Here again the interpretation is doubtful. If the blue were seen as a color, one would have supposed that some signs of an association between it and food should have begun to make their appearance by this time. If on the other hand the habit of choosing the darker impression as such had been formed, and grey number 7 had looked darker than the blue, then even more difficulty should have been experienced in learning to choose blue rather than grey number 15, which is darker than grey number 7, This increased difficulty seems actually to have been felt, judging from the results. The series lasted from March 24 to April 5.

Series 16. Red and grey number 7. Red opens. This series was given merely to re-establish the old red and grey habit in order that it might serve as a basis for some interpolated blue and grey tests. Eight experiments were given a day for five succeeding days: the percentages were 87, 62, 75, 87, 87, 100. The series lasted from April 5 to April 10.

Series 17. Red and grey number 7, with interpolated blue and grey 15 tests. If the grey 15 looked darker than the blue, and the rabbit were capable of forming a habit of reacting to the darker impression in general, then tests where grey 15 and blue were used, interpolated in series where the rabbit was choosing red rather than grey 7, should produce a preponderance of choices of the grey. On each day eight tests with red and grey 7 were given, then a test with blue and grey 15, then a red and grey 7 test, and then a final blue and grey 15 test. In all the 63 red and grey 7 experiments the rabbit went at once to the red. There were altogether 14 of the interpolated blue and grey 15 tests. In these the rabbit touched the grey first 11 times. She did not touch the blue first in any of the six tests made on the first three days. The most obvious explanation of this result is that grey number 15 looked darker to her than the blue: at the same time the number of experiments is too small to allow us to make this statement with certainty. It is a disadvantage of the method of interpolating tests that much time is required to gather a few results. The series lasted from April 10 to April 16.

Series 18. Stoelting black paper and grey number 7. Grey opens. This series was undertaken with a view to training Dark Nose to choose the lighter rather than the darker of the two papers presented, in order that further color experiments might be made with this reversed habit as a basis. We did not anticipate that it would take as long to form the habit of opening the grey door as proved to be the case. The experiments were made at the rate of ten a day. In all there were 387. The daily percentages of right choices, that is, choices of the grey, were 10, 10, 30, 40, 50, 50, 55, 41, 37, 0, 40, 57, 50, 50, 55, 50, 30, 30, 50, 80, 60, 80, 100, 90, 60, 80, 70, 60, 70, 70, 70, 70, 90, 90, 90, 60, 70. Evidently there existed to be overcome one of two tendencies: either a strong tendency to choose the darker of the two impressions presented, or a strong inhibition of the impulse to push grey, whose absolute character was somehow impressed on the rabbit's mind. The series lasted from April 17 to May 26.

Series 19. Red and Stoelting black paper. Red opens. This series was a final attempt to see whether the red-Stoelting black discrimination could be acquired. Ten experiments were made a day. The *total number of experiments* was 173, and the *total percentage of correct choices* was 48. The daily percentages were: 37, 77, 62, 50, 50, 40, 60, 40, 20, 55, 55, 33, 63, 45, 50, 50, 30. Twice in this series a well-marked right-hand tendency appeared and once a left-hand tendency occurred for a short time. Although the red-grey discrimination had been thoroughly learned, and as Series 16 and 17 show could be rapidly re-learned, no progress is shown in learning the red-black discrimination. This series lasted from May 27 to June 12.

C. Experiments with Abednego (*male*)

The three rabbits born in the laboratory were first tested on the twenty-fourth day of their lives. They were well able to eat lettuce and carrots by this time, and much interested in them. Their slowness in learning the box discrimination seemed due to the readiness with which they were distracted: their tendency on being put down on the laboratory floor was to wander about aimlessly. They were set down about one foot away from the experiment box, which, to prevent place associations and guidance by the smell of food left on the floor, was moved between experiments. From February 15 to March 11, tests were made on the three rabbits in rotation.

Series 20. Red and Hering grey number 7. Red opens. From February 15 to February 22, three tests a day on each rabbit were made. Abednego had a *total of* 45 *per cent. of correct choices* in the 33 experiments performed by him. From February 28 to March 11 four tests a day were made on each rabbit. Abednego made a *total of* 52 *per cent. correct choices* in the 44 experiments performed by him. On the last two days of this part of the series he made a total of 87 *per cent.* From March 11 to March 20 the four daily tests on each rabbit were made in immediate succession, that is, the four tests with Abednego were finished before those with Shadrach were begun, and so on. Abednego made a *total of* 86 *per cent of correct choices* in the 40 tests of this part of the series, and may be considered to have acquired the discrimination.

Series 21. Red and Hering grey number 15. Red opens.

Four tests a day, made in immediate succession, comprised this series. The *total number* of experiments was 96. Abednego made a *total of* 77 *per cent. of correct choices.*

Three times, on two successive days each time, a right-hand tendency appeared, and on one day every choice was of the left-hand door. The series lasted from April 11 to April 16.

From April 17 to April 23 Abednego was given ten tests daily. In these 60 experiments he made a total of 95 *per cent. of correct choices.*

Series 22. Red and Hering grey number 15, with interpolated red and Stoelting black and red and velvet black tests.

The plan was as follows. Eight tests were given on an experiment day with red and grey number 15, the red opening. Then two tests were made with red and velvet black. Next, two more red and grey tests were given, and finally two with red and Stoelting black. In the red and black tests we tried the experiment of leaving both doors unbolted, so that no punishment was involved for either choice. This was done on the theory that the rabbit would then make its red and black choices on the basis of the experience gained with red and grey, rather than on the basis of experience gained with the red and black combination itself. Dark Nose had given some indication of seeing the velvet black as darker than the red, and the Stoelting black had apparently been for Light Nose and Dark Nose of equal brightness with the red. If Abednego could be trained to choose the darker impression as such, he should choose the velvet black oftener than the red in these interpolated tests, while no difference should appear in the numbers of choices of red and Stoelting black. The 150 red and grey tests were made without a single error. In all, there were 26 interpolated *red and velvet black* tests. In these he chose the *black* 46 *per cent.* of the time and the *red* 54 *per cent.* There were 31 interpolated *red and Stoelting black* tests: the *red* was chosen 51.5 *per cent.* of the time and the *black* 48.5 *per cent.* of the time. The series offered no evidence that the velvet black was seen as darker than the red It is possible, though in view of the other results with red and black and of the unlikelihood that the rabbits could accumulate experience from a few experiments of one kind scattered among experiments of a different kind, not at all probable, that the absence of discrimination between red and black was due to the fact that both doors were open in these red and black tests.

. A special treatment of the results of the red and black interpolated tests was made to investigate the possibility that the perfect record which the rabbit was making with the red and grey was due to the formation of a habit of going to either side alternately, instead of to a visual discrimination. If such an alternating habit was being followed in the red and grey tests, then when a red and black test was introduced into the series the rabbit should have had a tendency to choose the door on the opposite side to that chosen in the experiment immediately preceding. That such was not the fact is shown by the following figures. Out of a total of 57 interpolated red and black experiments, in 47 *per cent.* the rabbit went to the *same* door that he had opened in the preceding test, and in 53 *per cent* he went to the *opposite* door. Evidently his perfect series of choices had nothing to do with a tendency to alternate from side to side. The series lasted from April 23 to May 8.

Series 23. Saturated blue and Stoelting black paper. Black opens. The rabbits had been unable to discriminate red from Stoelting black paper, and the only rabbit so far tested with blue and grey 7 and 15 had not learned to discriminate them. The combination of Stoelting black paper and blue paper should then be equivalent, if the black door opened, to the ordinary red-grey combination, the red door opening. Ten tests a day, on May 10 and 11, were given Abednego with the blue and black, and he chose the black without hesitation in each of the twenty tests. Whether the blue looked like grey to him or not, it is probable that the red looked like black.

Series 24. Blue and Hering grey number 15. Blue opens. This was a series of twenty tests only, ten on May 12 and ten on May 13. The blue door was pushed only 30 *per cent.* of the time, seven out of the ten choices on each day being of the grey. It is difficult to account for this sudden tendency to choose the grey, which had not been once touched for a long period, except on the supposition that the rabbit was continuing the habit of choosing the darker impression, and that grey number 15 looked darker to him than blue. One other possibility is that he was avoiding the blue for some reason. But inasmuch as his previous experience with blue had been confined to twenty tests with blue and black, in none of which he had touched the blue, his avoidance could not have been on the basis of experience gained in the experiments. It might have been innate. And the choices of the grey might, in view of the small number of experiments, have been accidental.

Series 25. Blue and Hering grey number 7. Blue opens. This series lasted from May 15 to May 22, a period which included some abnormally hot weather, during which the rabbits were languid and did not work well. There were 76 experiments in all. The *total percentage of correct choices* was 48. The percentages for each day, ten experiments being made on all the days but one, were 40, 30, 70, 60, 60, 33, 50, 50. Unless this record is due to the hot weather, the combination presented great difficulties in the way of discrimination.

Series 26. Blue and Hering grey number 2. Blue opens. This grey is next to the lightest in the Hering series, and to human vision is almost white. The series lasted from May 23 to June 6. One hundred and fifty-four experiments were made in all. Unfortunately we had reached a period where the young rabbits no longer responded satisfactorily to the tests, whether this was due to the arrival of hot weather or to some other cause. Abednego developed in the course of the series many right-hand and left-hand tendencies. Finally he formed an elaborate habit, which consisted in going to the right end of the box, strolling completely around the box, coming back to the front, and pushing the left-hand door. Sometimes he would turn back on his course before getting all the way around, and then he would push the right-hand door, again the first one he encountered. This habit was broken up by putting the box in a corner of the room, but the rabbit seemed to have lost interest in doors and in the box as a whole, so that we felt little confidence in the results. There were, however, six successive days on which no place tendency appeared, and on these the total percentage of choices of the blue was 63, the percentages of the several days being 50, 70, 70, 80, 60, and 66. There is therefore some indication that blue can be discriminated from grey number 2, either as a color or because it looks darker than the grey.

D. *Experiments with Shadrach* (*male*)

Series 27. Red and Hering grey number 7. Red opens. This series was made during the same period as Series 20, and under the conditions described in the report of that series.

In the 33 tests performed at the rate of three a day, the rabbits being tested in rotation, Shadrach made a total of 67 *per cent.* right choices.

In the 44 tests made at the rate of four a day, Shadrach made a total of 61 *per cent.* of right choices.

In the 40 tests made at the rate of four a day, the tests on one rabbit being completed before those on another were begun, Shadrach made a total of 67 *per cent.* of right choices.

No experiments were made on this rabbit from April 16 to May 8. On May 9 and 10 he was given 20 tests, 10 a day, with red and gray number 7, and chose the red every time.

Series 28. Saturated blue and Stoelting black paper. Black opens. This series corresponds to Series 23. Ten experiments were made a day, with the exception of one day on which only six were made. In the 56 tests, Shadrach chose the black 91 *per cent.* of the time. His results thus confirm, in a longer series, those obtained from Abednego. The series lasted from May 10 to May 16.

Series 29. Saturated blue and grey number 7. Blue opens. This series comprised 287 experiments, made for the most part at the rate of ten a day. His *total percentage of correct choices* for the series was 57. His daily percentages were as follows: 20, 30, 50, 50, 50, 60, 80, 50, 80, 60, 50, 100, 50, 50, 70, 40, 80, 60, 45, 100, 55, 60, 80, 80, It would appear that some progress was being made towards learning this discrimination.

E. Experiments with Meshach (*male*)

Series 30. Red and Hering grey number 7. Red opens. This series was made during the same period as Series 20 and 27, and under the same conditions.

In the 33 tests made at the rate of three a day, the rabbits being tested in rotation, Meshach made 40 *per cent.* of right choices.

In the 44 tests made at the rate of four a day, Meshach made 47 *per cent.* of correct choices.

In the 40 experiments made at the rate of four a day, without rotation, Meshach made 77 *per cent.* of right choices.

Series 31. Red and Hering grey number 15. Red opens. This series corresponds to Series 21. In the 96 tests of the series, made at the rate of four a day, Meshach made a total of 60 *per cent.* of right choices. His behavior was very irregular, and on two days work with him had to be given up altogether. On the last three days his percentage of correct choices was 58, and on the preceding three days it was 33. The work with this rabbit was interrupted from April 17 to May 8. From May 8 to May 17 he was given ten tests a day, in immediate succession, with red and grey number 15. In the 90 experiments his total percentage of correct choices was 88. Increasing the number of tests a day seemed to steady his otherwise highly erratic behavior toward the experiment box.

The refusal of Meshach to have any further dealings with the box after four days' trial of the red-Stoelting black combination has already been recorded.

F. *Experiments on Polly (female)*

Series 32. Red and Hering grey number 7. Red opens. These experiments were made in the yard attached to the hutch, under the same conditions as the experiments with Light Nose, that is, the rabbit was admitted to the yard from the hutch for each test, and driven back into the hutch between tests. She learned to run back of her own accord after a test. If any disturbing factor was involved, in the experiments carried on indoors, by the fact that the rabbits were dropped in front of the experiment box, it was absent here and in the work with Light Nose, for these rabbits were usually not touched between tests. The series contained 260 experiments. The daily percentages were as follows: 60, 55, 50, 70, 50, 61, 56, 65, 50, 95, 60, 60, 70, 100, 90, 80, 70, 80, 60, 100, 90, 80, 100, 80, 90.

The series lasted from October 3 to October 29, 1911.

Series 33. Red and Hering grey number 7, with interpolated red and velvet black tests. We wished to see whether this rabbit would confirm the results of Dark Nose in showing a preference for velvet black over red, or those of Abednego in showing no preference between the two. The red door was open and the black door closed in the interpolated tests. Six red and grey tests were given, on an experiment day, in succession; then four red and velvet black tests alternating with red and grey tests. In the 40 red and grey tests the grey was chosen only once. In the 16 interpolated red and velvet black tests the *red* was chosen 9 times and the *black* 7 times. There was thus no confirmation of the results obtained from Dark Nose. This series lasted from October 30 to November 2.

Series 34. Red and Hering grey number 15. Red opens. There were 50 experiments in the series, made at the rate of ten a day. The daily percentages were 60, 80, 80, 100, 100. The series lasted from November 3 to November 8.

Series 35. Hering grey number 15 and white. Grey opens. This series comprised ten experiments only; it was interpolated to see whether the rabbit could follow the habit of choosing the darker impression when the absolute brightness of the two impressions was altered. The red was presumably darker than grey number 15, and the rabbit had learned not to push this grey when shown with red. Would she push the same grey when it was shown with white, thus appearing as the darker of the pair? The white paper used was white letter-paper with a dull finish as nearly like that of the grey paper as we could find. The rabbit chose the grey door in each of the ten tests.

Series 36. Red and Hering grey number 15. Red opens. This series was given to re-establish the habit of choosing red rather than grey. If the rabbit were capaple of forming a steady habit of choosing the darker of the two papers presented, irrespective of their absolute brightness, the red-grey discrimination would not need to be re-established after the grey-white series, since in both sets of experiments the darker paper was the one to be chosen. As a matter of fact the red-grey discrimination was not so good after the grey-white series had been made as it had been immediately before. Ten experiments a day for three days, November 10, 11, and 12, were made, the percentages of choices of the red being 80, 70, and 80.

Series 37. Tests with red and grey number 15, the red open, intermingled with tests with grey number 15 and white, the grey opening. This series followed a new plan. Twelve experiments were usually given on a day. Six of them were red and grey 15 tests, the other six were grey 15 and white tests. Two of a kind were usually given in succession, the open door being on opposite sides in the two. The grey door was closed if shown with red, open if shown with white. The open door, though on opposite sides in the two succeeding tests of the same kind, was sometimes in the next following test on the same side as in the one immediately preceding, and sometimes on the opposite side: that is, the alternation of the sides on which the open door was to be found was not continued through the whole series.

The total number of *red and grey* experiments was 77; in 73 *per cent*. of these the *red* was chosen. The total number of *grey and white* experiments was 70; in 72.8 *per cent*. of these the *grey* was chosen. The percentages calculated every three days, that is, for (usually) every 18 tests of each kind, were as follows: choices of the red in red-grey tests, 74, 73, 68, 76; choices of the grey in grey-white tests, 73, 78, 86, 52. On one day, November 20, a perfect record was obtained in the whole twelve experiments.

It does not seem possible to explain the high percentages of choices of the grey in the grey-white tests, the very same grey which the rabbit had learned to avoid in the red-grey tests, except on the supposition that to a certain extent a habit had been formed of pushing the door which carried the darker paper, irrespective of the absolute brightnesses involved. Yet the absolute brightnesses were apparently not wholly without effect. In the latter part of this combined series the rabbit began to show signs of confusion. Further, as has just been noted, if the habit of reacting to the darker impression as such had been perfectly formed, the introduction of grey-white tests ought not to have had an unfavorable effect on succeeding red-grey tests. That such an effect probably was exerted is shown by the results of the next series. Series 37 lasted from November 13 to November 24.

Series 38. Red and grey number 15. Red opens. This series lasted from November 25 to December 6. There were 105 experiments in the series, made, with the exception of one day, at the rate of ten a day. The *total percentage of correct choices* was 62. The percentages on successive days were 60, 90, 30, 60, 70, 35, 80, 70, 56, 80. The rabbit's behavior suggested that the difficulty lay in the fact that the grey, whose absolute character was somehow recognized, sometimes set off the impulse to push it, as in the grey-white tests of the preceding series, and sometimes inhibited the impulse, as usually in the red-grey series. When the animal started in the direction of the red, she ran to it and pushed without hesitation; when she started for the grey she commonly drew back or stopped on the way to it, either finally pushing it after doing so, or turning to the other side and pushing the red.

Series 39. Red and Hering grey number 46. Red opens. The paper called "Stoelting black" in preceding series was, as has been said, nearer in brightness to Hering grey number 46 than to any other of the Hering greys. This last series was undertaken to find whether Polly, working with red and gray number 46, would get results like those obtained by Dark Nose, Light Nose, and Abednego in their experiments with red and Stoelting black paper. There were 180 tests in the series, twelve experiments being made on a day. The series lasted from December 7 to December 22. The *total percentage of correct choices* was 52. The daily percentages were 58, 58, 58, 58, 50, 58, 50, 58, 66, 50, 50, 66, 42, 50, 66. This series of figures may fairly be compared with those from Series 32, with red and grey number

7. Although Series 32 was the first which Polly made, and involved getting acquainted with the apparatus and the situation in general, she had made decided progress towards learning the discrimination at the end of 180 tests: her percentages for the first eighteen days were 60, 55, 50, 70, 50, 61, 56, 65, 50, 95, 60, 60, 70, 100, 90, 80, 70, 80.

It may therefore be concluded that like the other rabbits tested, she found it impossible, in the time allowed, to learn to distinguish between red and a grey of the brightness of Hering grey number 46, that is, a grey dark enough to be called black.

Supplementary tests on binocular vision

The rabbit has a small field of binocular vision, if we judge by the angle through which both eyes can be seen at the same time, a test which Waugh has applied to the mouse. In order to get some light on the question as to how much use the rabbit makes of binocular vision as compared with monocular vision, we made some simple tests on Polly. When she was sitting still, two bits of lettuce of equal size were held out, one straight in front of her nose at a distance of three or four inches, the other in line at right angles, directly in front of her left or right eye, and at an equal distance. Out of fifty trials the rabbit only twice took the bit of food held in front. The other forty-eight times, after sitting motionless a few seconds, she turned her head to the side and took the food offered there. It would appear that her actions were determined rather by monocular than by binocular vision. The rabbits seemed from our general observation of them to be influenced by binocular vision to a certain extent in their movements: that is, they seemed to avoid by vision objects directly in front of them. We often noticed in the box experiments, however, that a rabbit would turn its head from side to side before making a choice: sometimes a record like the following would be made: "Rabbit starts towards red, stops, looks at grey, turns back to red and pushes it." The experiments on Polly, just described, indicate that when a rabbit turned its head so as to face one of the doors, it was really looking, monocularly, at the other door. Thus the looking at grey described in the record was probably merely a turning of the head so as to get a better, that is, a monocular, view of the red before pushing it. And in turning the head from side to side, the rabbit is really looking at the object *from* which its face is turned.

IV. CONCLUSIONS

1. The rabbit can discriminate Bradley saturated red paper from Hering grey number 7 (Series, 2, 5, 8, 11, 16, 17, 20, 27, 30, 32, 33); from Hering grey number 15 (Series 1, 6, 7, 12, 13, 21, 22, 31, 34, 36, 37, 38), and from Hering grey number 24, though few tests were made with this grey (Series 3 and 9).

2. That this discrimination is a true visual discrimination and not one based on smell or some other clue; further, that it is based on brightness rather than on color difference, the red being seen as darker than the grey, is indicated by the fact that the rabbits gave no evidence of ability to discriminate red from "Stoelting black" paper or from the very dark Hering grey number 46 (Series 4, 10, 19, 22, 39), while the ability to discriminate was again shown as soon as a lighter grey was used (Series 5, 7, 22).

3. One short series (Series 13) suggested that Hering velvet black looks darker to the rabbit than Bradley saturated red. Series 22 and 33 failed to confirm this, and gave no evidence that red was discriminated from Hering velvet black paper.

4. Red has a very low stimulating effect on the rabbit's retina; a result which corresponds to what Yerkes has found for the dancing mouse and Watson for the monkey. Our experiments furnish no evidence that red is seen as a color: on the other hand they do not prove that it is not so seen. The rabbits may simply have found it impossible during the period of the training to base discriminations on color rather than on brightness differences. Whether color difference exists or not, the rabbit evidently attends more readily to a brightness than to a color difference.

5. The results regarding the brightness value of saturated blue are not conclusive. It seems to be readily distinguished from "Stoelting black" paper (Series 23 and 28), a discrimination which may be based either on brightness or on color difference. That the difference involved is mainly one of brightness, the blue looking lighter than the black, is indicated by the fact that blue and grey 7 were not discriminated in the course of 51 experiments by Dark Nose (Series 14), nor in the course

of 76 experiments by Abednego (Series 25). There is some evidence that grey number 15 looks darker than saturated blue: Dark Nose showed a marked tendency to choose grey 15 rather than blue in tests interpolated among red and grey tests where she was choosing the red, that is, the darker impression (Series 17); Abednego showed a tendency to choose grey 15 rather than blue in a series of 20 experiments following on a long experience of choosing the darker impression (Series 24). There was some evidence that saturated blue can be discriminated from the very light, almost white, Hering grey number 2, whether through brightness or color difference (Series 26). On the whole, it is suggested by our results that the brightness equivalent of saturated blue is in the neighborhood of Hering grey number 7, that is, decidedly lighter than would be the case for the human eye. It is interesting to note that one of Breed's chicks, which preferred white to black and yellow to black, preferred Bradley saturated blue to a yellow which was " very light, much brighter than the comparatively dark blue, not only as judged directly by the human eye, but as tested by the flicker method."[6]

On the other hand, the rabbit Shadrach, in next to the longest single series of tests made, showed some evidence that he was learning to discriminate saturated blue from grey number 7 (Series 29). If the discrimination was being acquired, it may have been based on a very slight brightness difference, or on a color difference.

6. When discrimination became difficult for the rabbits they fell back upon a habit of always choosing the door on one side of the box. Out of 39 instances where such a habit was recorded, it was a left-hand habit in 9 cases only, and in all these left-hand cases the habit persisted for one day only, while a right-hand habit often appeared for several successive days.

7. The rabbits did not acquire a habit, based on kinaesthetic data, of alternating from side to side in their choices, but were throughout guided by visual clues. This is shown by several facts.

a) No matter how regularly they had been choosing red in red-grey series where the red was on either side alternately, the red habit immediately broke down when black was substituted for the red. This was especially striking in Series 4 and 10, where, when the red and black tests were begun, the interval between successive tests on the same animal was greatly reduced, a circumstance which should have strengthened a motor habit of alternating from side to side, whereas in fact the alternation which had occurred while red and grey were being used now gave way to a habit of choosing the door on one side only.

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[6] Breed, F. S. The Development of Certain Instincts and Habits in Chicks. *Behavior Monographs*, vol. I, no. 1. 1911.

b) When in the midst of red and grey series in which red was being regularly chosen as it occurred on alternate sides, single tests were interpolated where black was substituted for grey, the tendency to choose the door on the same side as the one chosen in the preceding test was about as strong as the tendency to choose the door on the opposite side (Series 22).

c) In series where a high percentage of correct choices was being made the rabbits would often move first in the direction of the wrong door, checking themselves while a foot or more away and turning to the correct door. This would hardly have been the case had a motor habit been formed.

8. The results were not based merely on the formation of an inhibition of the impulse to push the grey door Breed, in his work on chicks, found evidence that in color tests they were not so much learning to choose one door as to avoid the other. Thus, after they had learned to choose black instead of blue, they chose white instead of blue, indicating that the discrimination was not based on a brightness difference, but rather on an acquired avoidance of blue.[7] Is it possible to explain our results with red by supposing that the rabbits learned in the red-grey experiments to avoid grey, rather than to choose red as the darker impression, and that they failed in the red-black tests because the grey, to which they had learned to check reaction, was no longer before them, so that their impulses were equally divided between the two doors which they saw? Undoubtedly the learning did involve the acquired inhibition of an impulse to push the wrong door: not infrequently, as we have said, the rabbit would run to within a short distance of the wrong door, stop abruptly, and turn to the other one. At other times, for long periods, the animal would dash at once to the right door as if the sight of it set off the impulse instantly. A careful examination of our results shows that the difference

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[7] *Op. cit.*

between the figures for red and grey and those for red and black cannot be due simply to the absence of grey in the red-black experiments. In Series 19, Dark Nose was required to learn to choose red rather than Stoelting black paper She had previously been trained to choose grey number 7 rather than Stoelting black. This training, which had been continued for 387 experiments, had resulted in an average of 75 per cent of right choices in the last 180 tests On the theory that the rabbits were learning to avoid the wrong door in itself, rather than to choose the darker or lighter of the two impressions presented, this black-grey training should have brought about a certain tendency to avoid the black, and inasmuch as, in the next series, with red and black, the red door opened, the rabbit ought to have continued to make a high record of correct choices, since all that was necessary was to continue to avoid the black. But we find that her total percentage of correct choices in this black-red series, comprising 173 experiments, was only 48. It is difficult to explain this failure to continue the process of learning to avoid black, except on the supposition that the rabbit found it practically impossible to distinguish red from black. The details of her behavior for the first three days of this black-red test are interesting In the preceding series she had been going for the most part without hesitation to grey, though occasionally making a wrong choice Our records read as follows for the first day of the final red-black series.

" 1. Red left. Put down before the box, rabbit runs away. Start over again. She goes near red, looks at it, goes around to left end of box and sits down. Start again. She noses red and black alternately Gets food from red.

" 2. Red right. Looks intently, goes to black [which she should have been learning to avoid], barely touches it, cautiously pushes red.

" 3. Red left. Goes to middle, pushes red, shaking ears.

" 4. Red right. Looks at box, dashes off to left, noses black, then red.

" 5. Two trials, but rabbit runs away each time."

The next day's work led off with six successive choices of the red. Two choices of the black follow, and then two more of the red. It looked as though there might have been here some recognition either of the red as the door to be chosen, or of the black as the door to be avoided. But on the next day the records read as follows.

"1. Red left. Towards red, pause, pushes red.

"2. Red right. Red.

"3. Red left. To black, pushes, shakes ears, to red.

"4. Red right. To middle, to red.

"5. Red left. Starts towards red, turns and faces black, stretches so that whiskers just touch black, shakes ears, pushes red.

"6. Red right. Touches black, to red.

"7. Red left. Went to left side, then to red.

"8. Red right. Went towards red, pauses a long time, finally pushes."

And in the ten experiments performed the next day she chose the right hand door every time, although with much hesitation and ear-shaking. There was no evidence from this time on in the series that red and black were distinguished.

As further proof that something more than an acquired avoidance of a particular visual impression *as such* is involved in our experiments, we may take the behavior of Dark Nose in Series 17. Here she was given daily 9 tests for seven days with red and grey number 7, in which she never failed to choose the red. In each day's series there were interpolated two tests with blue and grey number 15, and in these 14 experiments the rabbit touched the grey first 11 times. This does not look as though she had been avoiding the grey as such in the grey-red tests. It is true that the same grey was not used in the red-grey and blue grey experiments, but if we are to explain all our red-gray series on the supposition that they involved avoidance of grey as such, we must suppose that experience with one grey was transferred to the other greys, for Series 2 and 8, with red and grey number 7, gave high percentages of correct choices which could be accounted for only by supposing them due to the experience gained in Series 1 and 7: either the rabbits had there learned to choose the darker impression, or to choose the red as such, or to avoid grey, whether number 7 or number 15, as such.

Again, Abednego, after long training to choose red rather than grey, was offered the choice between blue and black, and invariably chose black (Series 23). If his preceding training had taught him simply to avoid grey as such, we should explain this behavior by saying that blue looked like grey to him here. But in the next series he was given blue and the grey (15) with which he had had most preliminary training, and he chose the grey 70 per cent. of the time.

On the whole, then, the consideration of the possibility suggested by Breed's work does not require us to modify our conclusions. Doubtless the reason why positive avoidance of a given impression as such plays more part in his experiments than in ours is that he used a much stronger punishment for wrong choices than we did; his chicks were given electric shocks, while our rabbits merely pushed against closed doors.

9. To a certain degree, the rabbit is able to form a habit of choosing the darker of two impressions, irrespective of their absolute brightness. It has been noted that we have found such a supposition useful in explaining, at several points, the behavior of our rabbits in a new series as due to their experience in previous series. No other hypothesis seems conceivable to explain the results of Series 37, where Polly chose grey 15 72.8 per cent of the time when it was presented with white, and only 27 per cent. of the time when it was presented with red. But the rabbit has no grasp such as human beings would gain of the *principle* of choosing the darker impression: the absolute brightnesses involved have a disturbing influence, as suggested by the fact that ordinary red-grey series, presented after the complex series, (Series 38 after 37), and after a short grey-white series (Series 36 after 35) showed a falling off in the percentage of correct choices, apparently due to the association of conflicting impulses with the grey as such.

10. There is evidence to indicate that while probably capable of binocular vision, the rabbit uses monocular vision in preference.

MAGNETIC CONTROL OF GEOTROPISM IN PARAMOECIUM

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In a preliminary paper the writer showed that cultures of Paramoecium caudatum which have ingested iron particles show an increased negative geotropic response for a temporary period. It was observed that the particles are at first lodged in the posterior end, and that the changed response is coincident with this condition and tends to disappear as the particles become more evenly distributed through the endoplasm. In an animal so loaded the posterior end tends to sink more rapidly so as to cause it to assume an upward orientation, provided that it is moving in a narrow spiral or nearly straight path, i. e., in the absence of any marked deflections of the anterior end such as occur when the animal is stimulated. This is readily observed, particularly in individuals which start to move in a horizontal direction. The action is to be interpreted as a passive orientation to gravity not involving response to a stimulus.

This result naturally suggests the possibility of experimenting with the magnetic control of the movements of Paramoecia containing iron. In this paper follow some experiments with the combined effects of gravity and magnetism.

APPARATUS AND METHODS

An electro-magnet was employed wrapped for a length of 27.5 cm. with seven turns of wire, 57 turns to the cm. The core was of ten pieces of a diam. of 4.5 mm., 31 cm. in length. An alternating current from the lighting circuit was used, which was passed through a 50 w. incandescent lamp in series. A direct battery current was also tried for comparison, but its use gave no different results.

A half-gram or so of iron by alcohol is rubbed up fine in an agate mortar with a few drops of water. From 1-2 cc. of water containing a dense culture of Paramoecia, free from sediment, is added and the iron kept in suspension by drawing the water in and out with a pipette; or the whole may be placed in a small vial and turned back and forth. After this treatment the Paramoecia may be recovered without the iron by pouring the whole into a Syracuse watch glass with water and the Paramoecia removed with a pipette by several rinsings. They were then placed for experimentation in a flat rectangular jar about 3 x 1½ x ¾ inches. This was supported with its narrow face against the core of the magnet. The water used is preferably boiled to drive away gases. The normal geotropic reaction of Paramoecium, as Jennings points out, is a delicate one and is best elicited in pure water in the absence of disturbing chemical factors and mechanical agitation. But as pointed out in the previous paper ingestion of iron accentuates the geotropic response so that it is not so readily masked by chemical and other factors.

EXPERIMENTS

1. *Ingestion of iron for a relatively long period, five minutes or more.*

Three controls are needed in connection with this experiment. First a culture of normal Paramoecia in a similar vessel and subjected to similar shaking, as mechanical agitation has a tendency to drive them to the bottom. The control Paramoecia used in this set of experiments were taken from a dense ring round the edge of the water in the stock jar. They showed a slight normal upward tendency after a time, but were apt to remain rather evenly diffused and finally to settle all over the walls of the jar, which was probably due to the large extent of its inner surface. Other cultures sometimes display a tendency to go downward or more commonly aggregate toward the top and occasionally form a ring.

As a second control such a jar of Paramoecia may be brought into the field of the magnet, which gave no effect, and would disprove the existence of anything in the field itself to influence the movements.

Specimens of the treated culture under the microscope showed a heavy load of iron particles. The behavior of such a culture without the magnet furnishes the third control experiment. These moved downward and soon left the upper third of the jar fairly free from Paramoecia. A considerable number would come to rest on the bottom where they would remain for short intervals before rising. After the lapse of a half hour, more or less, such a culture as this aggregates rather densely toward the bottom, presumably as a result of ciliary fatigue. After 24 hours, or even less, they may all be found risen again and apparently no worse for the treatment.

When such a treated culture is placed in the field of the magnet, as soon as the current is turned on there begins a strong upward streaming past the pole and as a result a downward movement also becomes manifest on the side away from the magnet. The downward movement is wider and more diffuse than the upward. The latter was a narrow stream up to a half inch in width. The concentration of this upward streaming gives it a striking appearance, while the downward movement is more scattered. Thus the culture shows the decided appearance of being in a state of circulation. Some of the Paramoecia in the upward stream continue on up to the surface of the water, but more usually they begin to disperse in all directions. They may move to a short distance, meanwhile sinking, and return to the upward stream, or they may go farther away before they finally sink. A considerable aggregation is found in the outside lower corner, i. e., away from the magnet.

As to the explanation of the upward streaming, it is evident that it involves explaining the upward orientation of the animals and their ability to rise in the stronger part of the magnetic field, while elsewhere they tend to sink. It is apparent that the movement upward arises mainly from below the pole and also from the side, and that it consists chiefly of upwardly oriented animals which do not congregate at the pole, but rather move more freely the nearer they are in its vicinity, and frequently keep on going upward. They appear therefore to experience a greater buoyancy in the magnetic field, so that we may assume the effect of the magnet to be due to its diminishing the effect of gravity upon the heavily loaded animals. Their upward orientation is another matter and is explicable by gravity alone, based on the observation that the posterior end is of higher specific gravity than the anterior where the stronger cilia are located.

To better understand this we need to refer to Jennings' lucid description of the action system of the animal. When Paramoecium is unimpeded in its progress in any way it tends to

PLATE 1.

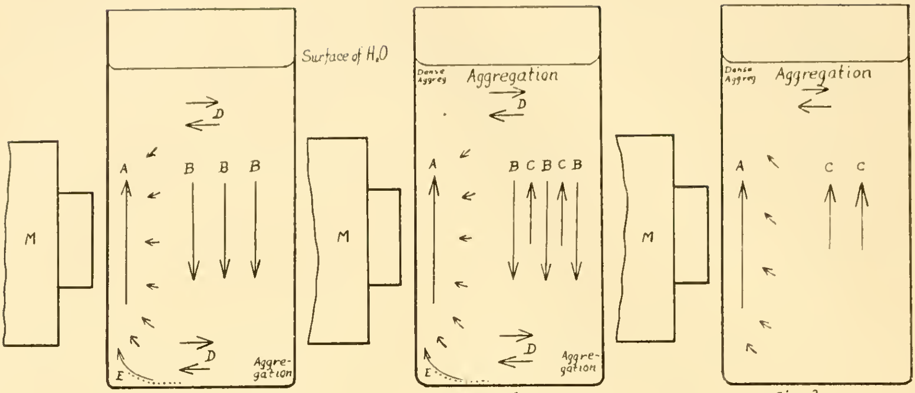

Fig 1 Fig. 2 Fig. 3

PLATE 2

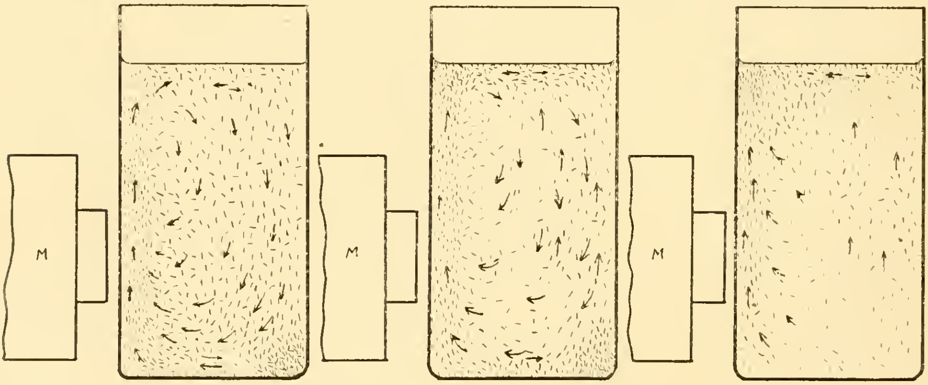

Fig. 1 Fig 2 Fig. 3

Plate I. Figures 1, 2, 3, refer to experiments in the text of the same number.

The arrows indicate the oriented movements upward, downward, and toward the magnet.

Random movements of dispersal from the top of the upward stream, and at all other points, are not represented.

A, upward oriented movements in the region of increased buoyancy.

B, heavily loaded animals moving downward in the weaker magnetic field.

C, upward oriented animals consisting presumably of those which are less heavily loaded. These tend to form the aggregation at the top which appears in (2) and (3), but not in (1).

D, horizontal paths of animals very close to the water surface or to the bottom, which appear to be governed by nearness to these surfaces.

E, individuals lying quiescent on the bottom, drawn passively at first toward the magnet.

The small curved arrows represent paths toward the magnet. The animals swing gradually into the upward stream. The arrows are curved to indicate this fact. Plate 2 illustrates the same facts by another method.

move in a very narrow spiral or nearly straight path. In this condition it is easy to observe that the tendency of the posterior end to sink and bring the animal into a position of vertical equilibrium with anterior end up is of very great effectiveness On the other hand when the animal is stimulated externally in any way the swerving habit entirely annuls the passive sinking tendency of the posterior end and becomes the chief means of orientation. The anterior end can orientate the animal actively, the posterior end only passively Now when a heavily loaded Paramoecium moves upward outside of the region of increased buoyancy, it meets with just such a difficulty in its movements as will cause it to swerve in a broad spiral and until the changed path is downward it will continue to move in that sort of a spiral, which, as Jennings shows, is highly adaptive for bringing the animal into a new direction where the stimulus to change will be lacking.

The movement toward the magnet is the most diffuse feature of the circulation, so that superficially it might be set down as the result of the random movements of the animals. Ocular demonstration of the existence of a definite orientation toward the magnet is apparently rendered difficult for two reasons. First, orientation toward the magnet is accompanied by a gradual orientation into the vertical position of equilibrium by sinking of the posterior end, so that the actual path traversed is a curve. The animals do not appear moving in parallel lines, which is quite a striking feature of the upward streaming. Second, there are two paths of less resistance, toward the earth and toward the magnet and considerable intermingling of the two effects. Evidence of a direct pull of the magnet is best seen upon those animals which may be lying quiescent upon the bottom just within its influence. These often appear to move at first as if dragged like passive objects.

The remaining feature of the circulation to be considered is the dispersion of the animals after they cease to go upward in the field of the magnet. At some point, which differs with different individuals, they begin to swerve and disperse horizontally. The further they move away, of course, the more likely they are to be influenced by gravity more than the magnet and return to the bottom.

2. *Ingestion of iron for a moderate period, two minutes.*

In such a culture as this it was found that a large number of the animals in a control orient upward and aggregate toward the top of the water. But a considerable number showed evidence of overloading and aggregated toward the lower part of the jar. Under the influence of the magnetic field, a culture of this sort showed the streaming movements distinctly. There was this difference, that an aggregation was formed overhead of the field of the magnet at the water surface. The animals which are oriented upward continue in many cases to the top and there may remain, often forming a dense ring at the water's edge. These are therefore an aggregation of active animals able to maintain themselves at the top, and contrasting with the aggregation in the lower outside corner, consisting of sluggish individuals. The latter sort, if near enough to the field of the magnet are sometimes noticed to move as if passively pulled by its attraction.

3. *Ingestion of iron for a brief period, 20–30 seconds.*

In this length of time only a light load is taken in. With the cultures used this produced a marked upward tendency in the control, so as to leave the lower region approximately free from Paramoecia. When this culture was brought under the influence of the magnet it appeared that the upward streaming was for a short time visibly greater on the side of the magnet, but there was no noticeable compensating movement downward, and in consequence the upward streaming was of short duration. The aggregation above the magnetic pole was distinctly greater than elsewhere, so that this case gives only a single region of aggregation.

THEORIES OF GEOTROPISM OF PARAMOECIUM

Various explanations have been offered for the geotropic response, such as sensitiveness to the difference in pressure between the upper and lower portions of the organism; sensitiveness to the greater resistance encountered in swimming upward than downward; response to internal localized stimuli by heavier particles in the endoplasm—the statocyst theory. All have taken for granted that orientation to gravity was brought about by a response to some sort of stimulus.

Verworn is credited by Lyon with having earlier held that the geotropic response of Paramoecium was explicable on the basis of the form and structure of the organism, a view which he is said to have later abandoned. The original reference is not at hand.

These theories are discussed by Davenport, with the exception of the later one of Lyon, and he credits the mechanical factor as having some influence at least in producing the geotropic orientation.

DISCUSSION OF RESULTS

In presenting what appeared to the writer to be valid reasons for accepting the view that in the case of experiments with loaded animals the geotropic tendency is a result of passive orientation rather than a response to any form of stimulus by gravity, I would not have it supposed that I am opposing the possibility of the action of gravity as a stimulus under any conditions. Inasmuch as evidence has been offered in this paper for the action of gravity as a stimulus upon heavily loaded animals tending to check their upward movement, it would be unwise to harbor a predilection against the possibility of its acting as a stimulus to account for movement in either direction.

Jennings instances the case of the geotropic collection of Paramoecia in distilled water which give the avoiding reaction on starting to move downward and so are oriented upward again. This furnishes an opportunity to compare my results with those of Jennings. In no case have I a record of a complete geotropic collection at the top of the tube in my experiments with iron filings and ordinary water. There have been aggregations toward the top with no sharp dividing line and the animals all show active swimming movements in all directions, except those which collect on the walls of the vessel and in the surface film. Hence there is no possibility of recognizing the geotropic response *per se* as there is in the pure form of the reaction described by Jennings. There is however abundant opportunity to observe the gradual swinging from a horizontal to a vertical path among such animals and the recognition of it as a prevailing type of movement. I have given the chief reason for using ordinary water in these experiments instead of distilled in which the pure response may be elicited. It is, to repeat, because the greater negative tendency of the properly loaded

Paramoecia is measured by comparing their behavior with an ordinary control. Control experiments in ordinary water may make quite complete surface aggregations after a considerable time, but usually the reaction is gradual, while the treated cultures move up often with surprising quickness. I have had cases of nearly complete surface collections of treated animals with only a few scattering individuals not included, but when these occurred after a half hour or more I was inclined to suspect the denseness of the aggregation was due to the formation of an acid area about them. Distilled water also has its disadvantages in causing their ultimate death and some amount of contraction of the body and sluggishness of movements. According to Jennings the avoiding reaction given when starting to move downward is the geotropic response *per se*. It would tend to produce an effect of "orientation by exclusion" to use his terminology. It appears to the writer that the mode of passive orientation would only supplement the geotropic response and not exclude it. The animal might swing passively into the oriented position during its ordinary movements and since this would be a position of stable equilibrium, it might happen under certain conditions,—say in pure water, that movement out of the position of stability might be able to induce a stimulus. Orientation by exclusion is not permanent. But a position of stability in the water does give permanence to the orientation and accounts for the quite regular movements in parallel lines, which is best shown under the magnetic control by heavily loaded animals in the upward stream.

Jennings also points out that resting animals on the bottom or in contact with the sides of a vessel or in the surface film all show characteristically the oriented position. This may surely be taken as corroborative evidence for the acceptance of the view that there is a position of equilibrium to gravity which is passively assumed. This fact accords well also with the other observation that the oriented position is assumed by animals moving quietly in narrow spirals and that any disturbance of their movements by stimulation annuls the passive orienting tendency. It must be remembered that the normal reaction is a very delicate one. As Jennings says: "Whatever the cause for the reaction to gravity, the stimulation it induces is evidently very slight and its effect is easily annulled by the action of other agents."

SUMMARY OF RESULTS

1. Paramoecia which have ingested a certain amount of finely divided iron show temporarily an increased upward orienting tendency which persists as long as the particles remain in the posterior end, and tends to disappear as they become scattered more evenly through the endoplasm.

2. The passive sinking of the posterior end is able to orientate the animal into a position of gravity equilibrium with anterior end up, only when it is moving in a narrow spiral or nearly straight path. Overloaded individuals which are impeded in their upward movements by the load begin to move in a broad spiral and so change their course eventually to a downward direction in the path of least resistance.

3. A magnet placed at one side of a jar containing Paramoecia which have ingested iron causes an upward streaming in the stronger part of the field. Those heavily loaded animals which move upward in this stream after dispersing above into the weaker part of the magnetic field tend to sink again and cause a return circulation to the bottom.

4. The magnet is effective in producing this circulation by diminishing the effect of gravity on animals containing iron. It also exerts a passive pull upon them and they gradually swing into their finally oriented position in a vertical path under the combined influence of the magnet and gravity. The oriented path is consequently a curve.

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FOOD AND CHEMICAL REACTIONS OF THE SPOTTED NEWT, DIEMYCTYLUS VIRIDESCENS

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Although Gage (3), Jordan (4), Monks (5), Pike (7), and others have published observations upon the habits of this common newt the writer is not aware of any experimental observations upon the feeding habits nor upon the chemical reactions in general of Diemyctylus.

The species is fairly common in the neighborhood of Morgantown, and about a dozen specimens, all in the green stage, were collected late in the autumn and kept in an aquarium in the laboratory, where the experiments described below were carried on. They were fed at intervals with earthworms and bits of raw meat, and seemed to remain in a perfectly healthy condition.

Merely as a matter of convenience the following descriptions will be given as a series of experiments.

Experiment I. The object of this experiment was to determine whether the animals found their food by sight or by smell, using the latter term to designate what Miss Washburn calls the "telaesthetic sense" of aquatic animals.

Most of the tests were made upon three animals that were selected because of their especially healthy appetites, although they had not been subjected to any especial starving process, but had been fed, or at least offered food, at the same intervals as the rest.

The first animal followed actively a small roll of filter paper held in fine-pointed forceps and seized it once or twice, but let go of it immediately. It reacted the same way towards similar rolls of white and of black raw cotton, but by this time seemed fatigued so that it scarcely noticed a roll of yellow cloth. A piece of raw meat of similar size and shape was now offered; it was immediately seized before it could be withdrawn and was held to so tightly that the animal was lifted entirely out of the water before it let go its hold.

A day or so later, after snapping again and again at a piece of meat, it half swallowed a piece of filter paper, but immediately disgorged it. It then seized and swallowed a piece of meat.

Two days later it followed and seized but immediately dropped a piece of yellow cloth; it then paid no attention to a roll of black cloth, but snapped a piece of meat when offered it.

The second animal was tested with the same small pieces of paper, cloth, etc. When offered the roll of filter paper, it followed it about in the aquarium quite actively, finally seizing and half swallowing it, but immediately disgorging. It then seized and completely swallowed a piece of white raw cotton, after which it refused to follow either black or yellow cloth, and only followed the meat after some coaxing.

Two days later it followed a piece of meat and snapped at it so suddenly that it was seized and swallowed before it could be withdrawn. It then seized and half swallowed a piece of filter paper, but disgorged it. A piece of filter paper soaked in meat juice that was now offered, was seized and swallowed; this filter paper was disgorged and was found in the aquarium two days later, at which time the animal followed and snapped at a roll of yellow cloth, but after once seizing it, refused to follow it again; a piece of meat that was now offered was followed and snapped at again and again, but was withdrawn each time before the animal could get it. A piece of black cotton which was offered was seized, but immediately disgorged. It now refused to follow a piece of white parraffin, but tried to seize a piece of meat when it was offered. The paraffin was seized by another animal, but was immediately dropped.

A third animal was tested in the same way as has just been described for the first two and acted in essentially the same way. It would follow small tasteless objects of various colors and would often seize them, but usually disgorged them almost immediately. Usually after snapping at the tasteless objects a few times the animals refused to follow them longer, but in such cases they would nearly always follow and try to seize a piece of raw meat of the same size. The color of the tasteless objects seemed to make little or no difference in the reaction.

Generally after swallowing one good-sized piece, whether of meat or of one of the indigestible objects, the animal could not be induced to touch anything more at that time.

The same three animals, and several others, were tried with a small piece of apple. Several of them followed the apple and one or two snapped at and half swallowed it, but it was always disgorged before it was completely swallowed. Several animals that paid no attention to the apple immediately followed and snapped at a piece of meat.

A small piece of caramel that looked almost exactly like the meat was followed and snapped at by some of the animals, but none of them would swallow it. Many of the animals that paid no attention to the caramel seized and swallowed, immediately after, a piece of meat.

The entire lot of animals was tried with pieces of meat and pieces of earthworm of the same size. Most of the animals seized, or tried to seize, the meat and earthworm, offered alternately, and no difference in reaction towards the two could be noticed. As in nearly every experiment, there were a few animals that refused to eat, and those that refused the one refused the other, in this case. None of the animals seemed to show any preference for the earthworm, a natural food, over the raw beef, food to which they were, of course, not accustomed in their natural haunts.

Several small pieces of meat and of earthworm were wrapped in filter paper and thrown into the aquarium with a number of animals. These small bundles, all looking alike, remained untouched for several days, when they were removed because they had begun to foul the water.

Experiment 2. As a further test to see whether the animals were guided by a sense of smell or taste, two kinds of beef extract were made; one by boiling meat in water and filtering the resulting broth, the other by grinding pieces of raw meat in a mortar with a little water and filtering the bloody liquid that was thus obtained. As the animals reacted the same to the two liquids, the latter was used in nearly all of the experiments. The method of using the meat fluid was to squirt a little of it from a fine-mouthed pipette upon the tip of the head of an animal that was lying still in the aquarium. As in Experiment 1, most of the trials were made upon three or four animals that responded more quickly than the rest, but all of the animals acted in essentially the same way.

When a little of the fluid was ejected upon the head of an animal it often responded by snapping its jaws, even after the pipette was withdrawn. If the pipette were held still, very close to the nose of the animal, it would often be seized by the animal and held in the mouth, just as were the tasteless substances in Experiment 1; this seldom happened when a clean pipette was held close to the nose of the same animal. No attention was paid to a jet of pure water squirted against the nose, nor to a jet of water in which cane sugar had been dissolved.

In many cases the fluid in the water, after the withdrawal of the pipette, caused the animal to put its nose to the bottom, in its characteristic way, as though searching for food; and with one animal the fluid not only caused this reaction, but it caused the animal to rub over the bottom with open mouth, and to snap, again and again, at spots in the glass bottom.

In some cases the pipette was held in such a position that it could probably not be seen by the animal, but the juice produced the same reaction as before.

In a few cases an animal which failed to respond to the meat fluid responded very quickly to a piece of meat, which would seem to indicate that sight is perhaps as important as smell and taste in securing food; later experiments, described below, seemed to disprove this, however.

Raw meat juice that had been allowed to stand until it had become rancid enough to have a decided odor, caused a negative response in nearly all of the animals. With these same animals that had withdrawn from the stale juice, the fresh juice caused in nearly every case a positive response such as has been described above—in one or two animals an extremely active snapping motion. The stale juice had no effect except upon the head of the animals.

An extract was made by grinding earthworms in a mortar with a little water and filtering the resulting fluid. This liquid was used in a pipette just as was the meat extract, first the meat and then the earthworm extract, but no difference in the reactions of the animals could be detected to show that they preferred one rather than the other.

In securing food, then, it is probable that Diemyctylus uses both sight and the telaesthetic sense, perhaps the latter more than the former. After the food has been located and seized, a chemical sense, taste or smell, usually decides whether it shall be entirely swallowed or not, though as has been seen above, the animal frequently swallows substances that would seem to have no taste or smell whatever. They sometimes seem to perceive by sight, objects, meat, etc., that are several centimeters distant; but the telaesthetic sense apparently operates only at much shorter distances.

REACTIONS TO LIQUID CHEMICALS

In the following experiments the reactions of the animals to all of the chemicals used, representing the various tastes as we know them, sweet, sour, bitter, etc., were tried in the same way; the solutions, in varying strengths, were squirted from a small pipette upon different parts of the animal as it lay quiet in the bottom of the aquarium in about five centimeters of water. As the animals are naturally quite active, especially in strong light, it was necessary to work in a rather dim light. Owing to this same activity, the animals usually became so restless after a few stimulations, that it was not possible to make a large number of tests at any one time; this made experimentation rather slow.

Since the animals are only about nine centimeters in total length, it was not possible to stimulate them in as many different places as was done by Sheldon (8) with the dogfish.

As a matter of fact, experiments failed to show, with the exception of the anterior part of the head, any greater sensitiveness in one part of the body over another. It was at first thought that the region of the base of the tail was more sensitive than any part except the head, perhaps because it was impossible, because of the small size of the animal, to stimulate that region without stimulating the region of the cloaca; but further experimentation failed to verify this early supposition.

The reaction to all of the chemicals used, with the exception of those described above under the head of food reactions, were negative. In the case of sugar only was there no reaction, and, in fact, even there a sort of negative reaction was seen, as will be described later.

The reaction when a chemical of sufficient strength was squirted upon the tip of the head, varied from a slow withdrawal backwards, when a very dilute solution was used, to a sudden withdrawal accompanied with a violent shaking of the head from side to side, when a strong solution was employed. If the stimulus were of unusual strength, the animal might roll over and over, as though in great agony, and then dart away to a distant part of the aquarium. Chemicals of this strength were seldom used more than once, because of the possible injurious effect upon the animal.

A weak stimulus on the hinder parts of the animal usually caused a slow motion forward to escape the stimulus, though occasionally a slight twitching was the only response; the twitching was usually followed, however, by the forward motion if the stimulus were continued. A more violent stimulus of this region caused a sudden start forward, and the animal very frequently reached back and bit at the spot stimulated. A still more violent stimulation produced not only the biting reaction, but caused the animal to roll over in apparent agony as was noted above in connection with the stimulation of the head.

A remarkable amount of variation was noted in the different animals in regard to their sensitiveness to chemical stimulation, both on the head and on the body regions. A stimulus that would cause one animal to respond in a violent way, as though it caused considerable pain, would have almost no effect upon another animal. The same animal at different times also showed considerable variation in sensitiveness, owing, it may be supposed, to variations in the physiological condition. It is possible that the variation in sensitiveness may be due partially to variations in the cuticle, though this could not be definitely determined. At any rate, the cuticle is shed with remarkable frequency, and it may be that when it is beginning to loosen preparatory to being cast, it may make the animal less sensitive to stimulation.

Experiment 3. To test the reaction of the animals to what is to us a sweet taste a 10% solution of cane sugar in distilled water was used. This concentration makes a very sweet solution, in fact almost a syrup, but no reaction proper was obtained by prolonged stimulation of either head or body regions, though all of the animals on hand were tried. That the solution was, however, not entirely without effect seems possible from the fact that while the animals in this experiment frequently snapped at and even seized the empty pipette or one containing pure water, they never acted in this way towards a pipette containing the sugar solution. It would seem that the sugar repelled them enough to keep them from snapping at the pipette, but not enough to cause them to withdraw from the jet of sugar solution when squirted against them.

Experiment 4. This experiment was to test the reaction of Diemyctylus to a bitter solution, and a nearly saturated aqueous solution of quinine sulphate was used. No reaction at all was obtained except when the solution was squirted upon the head. In this case there was sometimes a rather slow withdrawal, but usually the reaction was quite marked, the animal shaking its head in evident discomfort, or with what in the case of man would probably be disgust.

After reacting negatively to the quinine the animal would usually respond in the ordinary way to meat juice that was offered in the same manner. One animal that paid no attention to meat juice reacted, though not actively, to the quinine solution.

Picric acid was not tried because, as has been shown by Sheldon, the acidity is apt to cause a more marked reaction than the bitterness.

Experiment 5. As an example of a neutral saline solution sodium chloride was used. Nine animals were used, and a 4% solution of the salt in distilled water was first employed. This solution when squirted upon the head caused a negative reaction in all of the animals. In two or three the reaction was quick and violent, the animal shaking its head in evident discomfort. In others the reaction was quick but not violent; and in others the reaction was slow, taking place only after an entire pipette or more of the solution had been used.

On other parts of the body the 4% solution caused in all but one or possibly two, animals a long-delayed but usually marked negative response. In some animals the response consisted in simply a twitching of the part stimulated, in others there was a slow withdrawal from pipette, while in others the animals bit at the point stimulated; but even in the animals where the latter response took place, it was usually so slow that one or more entire pipettes of the solution had to be used before the reaction occurred.

A 2% solution caused definite responses in all but three of the animals when used upon the head, though the reaction was not so marked as with the 4% solution. No certain reaction was obtained with this solution upon other parts of the body.

A 1% solution gave a fairly quick though not violent response in the case of a single animal, used, of course, upon the head. Two other animals slowly withdrew from the stimulation after long delay. The remaining animals gave no response at all to this solution, which is probably about the limit of dilution for this salt.

Judging from the comparative reactions of the hydroxides of potassium and sodium it seemed likely that the animals would have reacted to the potassium chloride in the same way as to the sodium salt, so the former was not used.

It is of course evident that in all of these experiments the solutions are more dilute than is indicated by the time they actually come in contact with the animal, since they must be somewhat diluted by the surrounding water, even though the pipette be held as close to the animal as possible.

Experiment 6. To test the animals for alkalies solutions of sodium and potassium hydroxides in distilled water were used. The reactions obtained in the two cases were of the same character and, apparently, of the same intensity. The first experiments seemed to show a slightly greater sensitiveness to the potassium solutions, but later experiments showed that this was not the case. Since, therefore, the reactions were the same in the two cases, only the experiments with the potassium hydroxide will be described. Nine animals were used in this experiment.

A 2% solution of the potassium hydroxide when squirted on the head of an animal caused instant and extremely violent reaction, as though the animal were in great pain. A very small quantity of the solution was sufficient to cause this reaction, and in no other solution of the same strength was the reaction so violent. When this same solution was brought in contact with the body or tail of the animals the reaction was almost as quick and quite as violent as on the head, the animal writhing and biting at the spot stimulated as though in agony.

A 1% solution gave reactions upon head and body that were nearly as marked as those for the 2% solution, and a .5% solution gave nearly the same reactions for head and body, except that they were slower in starting.

All nine of the animals responded to a .25% solution, but more slowly and less violently than in the preceding cases. All nine animals responded fairly quickly and violently to a .125% solution when the head was stimulated; all but one responded when the tail was stimulated, and two of these bit at the spot affected.

All nine animals responded fairly strongly to head stimulation with a .0625% solution, though some responses were very slow. Three or four gave no response to tail stimulation, though one which did respond bit at the spot stimulated.

All of the animals responded to a .03125% solution on the head, though the response was very slow and usually not strong. Only one animal responded to stimulation of the tail, but that one bit at the spot stimulated as though the irritation were considerable.

All but two of the animals responded to a .015625% solution when the head was stimulated; the reaction was, of course, very slow. No reaction was obtained from any of the animals when the fluid was squirted against the tail.

A .0078125% solution produced a response in only one animal when the head was stimulated, and no response was obtained from any of the animals when other parts were stimulated.

Experiment 7. For reactions to acids various strengths of hydrochloric and glacial acetic were tried. Since no difference in the reactions was noted, except that, as might be expected the hydrochloric acid acted more strongly for a given concentration, the experiments with only the acetic acid will be described.

As was said above, experiments were tried to determine whether one part of the animal was more sensitive to chemical stimulation than another, but with the exception of the head, all parts of the body seemed to be about equally sensitive. Whether it were the nostrils, mouth, or eyes that caused the greater sensitiveness of the head it was difficult to determine, since, on account of the small size of the animals, it was almost impossible to stimulate one of these structures without the other. By carefully controlling the direction of the jet from the pipette it was possible to cause most, if not all, of the fluid to escape the eyes, and no difference was noticed in the reactions of the animals so stimulated from those in which the jet was directed upon the top of the head as a whole. The relative sensitiveness of the nostrils and mouth will be discussed later. By directing the jet caudad the eyes could be stimulated with probably but little stimulation of the nostrils and mouth; no certain difference in reaction could be determined from what was seen when the jet was directed in the other direction as noted above.

A 10% solution of glacial acetic acid was first used, and this was gradually diluted until no reaction was obtained; this dilution was made in all cases with distilled water. Twelve animals were used, the experiments extending over several days. The water was changed after each set of experiments.

The 10% solution, as might be expected, caused violent and almost instantaneous reaction when squirted upon the head, while upon the body it caused active though much less violent responses.

A 3% solution caused fairly quick reaction upon the body, and much more violent reaction upon the head, often a violent shaking of the head taking place. This dilution of the acid is distinctly sour but not irritating to the human tongue.

A 1% solution on the head caused a quick though less violent reaction than in the preceding. Upon the body it caused slow reactions, and in none of the dilutions lower than this one was there any certain response. It will be noticed how much more sensitive the animals are to weak solutions of alkalies than to weak acids. The 1% acetic acid has a faintly sour taste.

A .3% caused a moderately quick response upon the head, but, as was said above, no certain response upon the other parts of the animal. A finger moistened with this dilution of acid and touched to the human cornea caused a brief but decided stinging sensation.

A .075% solution caused in every animal a moderately prompt and decided response when squirted upon the head. When squirted upon the human tongue it causes a barely discernible sour taste, and in the human eye causes scarcely more sensation than would a drop of cold distilled water.

A .0375% solution caused a fairly quick and decided response in all animals, in a few cases with a shaking of the head in the manner characteristic of a disagreeable sensation.

A .01875% solution produced in all animals a negative response, generally fairly quick, and sometimes quite violent. This solution still has a faintly sour taste to the human tongue in quantity, but in the human eye produces no more sensation than cold water.

A .009375% solution produced in all animals a responses though not a very decided one. Squirted upon the human tongue this solution produces no sensation of acidity, though in larger quantities a faintly sour taste may still be discerned.

A .0046875 solution produced reactions in all animals, though in most cases so slow that several pipettes full had to be squirted upon the head before the animal withdrew.

A .00234375% solution, containing about one part of the pure acid to 42000 parts of distilled water, caused very slow responses, and is probably about the limit of dilution that will cause response, in these animals, though it is possible that under the proper conditions they might be still more sensitive. It is only by alternately drinking distilled water and the diluted acetic acid that a sour taste can be discovered in such a great dilution. Without the pure water for comparison the taste could not be detected, and it would not be noticed even in this case unless it were being tasted for.

Experiment 8. As a chemical that would not naturally be classed as sweet, sour, or bitter, but which produced a stinging sensation, alcohol was used.

10% ethyl alcohol, which produces a decided stinging sensation in the human mouth, caused a reaction with all the animals tested (nine) both upon the head and upon the tail. The head reactions were rather slow, and not as violent as might have been expected, when the violence of the reactions to weak acids and alkalies is remembered. The reactions from tail stimulation were usually even slower, as would be expected, though they were sometimes characterized by the biting motion mentioned above.

Only one of the nine animals reacted to 5% alcohol when the tail was stimulated. Only one of the nine failed to react to that grade of alcohol upon the head. Two of the latter reacted fairly promptly, the rest very slowly, only after one or more entire pipettes of alcohol had been squirted against the head.

None of the animals reacted to 2% alcohol when it was used upon the tail, and only four of them reacted when the head was stimulated. Of these four three reacted in a very slow uncertain way, while the fourth responded fairly promptly.

The only animal that responded at all to 1% alcohol was the one just mentioned as responding fairly quickly to the 2% grade; 1% is, therefore, probably the minimum percentage of ethyl alcohol that will cause reaction in these animals.

Experiment 9. The effect of cocaine was tried both to see what the effect might be, and to try to distinguish between the sense of taste and that of smell.

Thinking that the soft moist skin of Diemyctylus would be at least as sensitive to cocaine as the harsh skin of the dogfish that Sheldon used, a 2% solution was first tried. This concentration was found by Sheldon to produce insensibility first to touch and later to chemical stimulation. The cocaine was first tried by painting it over the nasal region of animals that had been taken from the water and allowed to dry for a few moments. In this process it was almost certain that the cocaine must have entered the nasal chamber through the nostrils. After a few minutes the animals were replaced in the water and were tested with a 1% solution of acetic acid, such as has been mentioned in preceding experiments, every ten minutes or more during several hours. The cocaine had no apparent effect whatever.

Thinking that possibly the sensitiveness of the head, after the nasal region had been treated with cocaine, might be due to stimulation of the eyes or the lips by the acetic acid, in spite of the care that was exercised to direct the stream from the pipette in such a direction that it should stimulate only the region that had been painted with the cocaine, the experiment was tried of painting the entire head, dorsal and ventral, including the eyes, with the 2% cocaine. The result was the same as before.

In other animals the cocaine of the same strength was used upon the tail; about half an inch of the tail, just caudad to the posterior appendages, was painted entirely round the animal and including the cloaca. This region was tested with the 1% acetic acid every fifteen minutes for several hours, but no change in its sensitiveness could be detected.

Another series of experiments in which a 5% solution of cocaine was used in the same way that the 1% had been used gave no difference in reaction to the 1% acetic acid. A 10% solution was then used in the same way, with the same negative result. It was then concluded that the skin of this salamander is, like the human skin, and unlike that of the dogfish, not susceptible to cocaine when applied superficially as may be done with a mucous membrane.

Experiment 10. In hopes of being able to distinguish between the sense of smell and that of taste a 5% solution of cocaine was used in the manner about to be described. Owing, however, to the fact that the posterior nares, as in the frog, are comparatively wide and are situated far forward in the roof of the mouth, it is probable that in this experiment the cocaine that was used in the mouth always affected the nasal chamber about as much as it did the oral mucous membrane; and that, as was said above, when the cocaine was applied to the anterior nares it leaked down into the mouth.

Since the anterior nares are very minute, scarcely large enough to admit an ordinary pin, it was useless to try to stop them up with cotton as did Sheldon (9) in his experiments upon the sense of smell in the dogfish.

The results obtained in this experiment were, as will be seen, somewhat conflicting, but they will be described as briefly as possible. An animal that snapped vigorously at a bit of meat was removed from the water and a small wad of raw cotton saturated with 5% cocaine, was put into its mouth and held there for about two minutes. The animal was then returned to the water and offered the bit of meat; it paid no attention whatever to the meat, though it was presented to its nostrils at intervals of from ten to sixty minutes during the next seven hours. It also responded very slowly and weakly to 1% acetic acid during the first half hour after the removal of the cotton from the mouth.

Another animal that also snapped quickly at a bit of meat, thus showing that it was hungry, was removed from the water and a wad of clean raw cotton, wet with pure water, was put into its mouth as was done with the cotton wet with cocaine in the first animal. On being replaced in the water this animal immediately snapped at the meat, and did so again at the end of ten minutes; this seemed to indicate that the raw cotton had no effect upon the feeding habits of the animal. The cotton was now soaked in 5% cocaine and again introduced into the animals' mouth where, as in the case of the clean cotton, it was held for two minutes. When the animal was returned to the water it paid no attention to the meat, nor to a 1% acetic acid solution. After five minutes it still paid no attention to the meat, but gave a weak response to 1% acetic acid. Half an hour later it was still indifferent to the meat but gave a fairly strong response to the acid. Two and a quarter hours after this it still refused the meat, but an hour later it again snapped at the meat, and continued to do so at intervals of an hour for the rest of the day, as though it had entirely recovered from the effects of the cocaine. The next day both of these animals refused to eat.

The third animal acted in about the same way as the two that have been described, except that on the second day it seemed to recover from the effects of the cocaine in a much shorter time than did the first two animals. After the first application of cocaine it failed to respond to meat for about three hours.

The fourth animal on the first day reacted as did the three preceding, but on the second day after removal of the clean cotton the animal refused to eat though tried at intervals for more than two hours. Whether the cotton had anything to do with this reaction cannot be said with certainty. Two days later this animal, after snapping at a piece of meat, was removed from the water and its nostrils were painted for three minutes with a 5% solution of cocaine, a camel's hair brush being used. On being returned to the water it refused meat for more than half an hour.

The fifth animal after removal of the clean cotton from its mouth refused meat for only a few minutes then snapped at it. It was then removed from the water and its nostrils painted, as in the preceding animal, with cocaine; it then refused meat for the rest of the afternoon, though offered it at intervals for more than two hours. Two days later this animal after being treated with cocaine in the same way snapped at the meat in less than half an hour after being returned to the water.

The sixth animal on three occasions, after snapping at the meat, was treated with the cocaine as in the preceding animal, i. e., had its nostrils painted with the 5% solution. On two of these occasions the animal snapped at the meat in half an hour or less; on the other occasion the animal refused food for the rest of the afternoon, about three hours.

The seventh animal on one occasion snapped at the meat immediately after being returned to the water; at another time it snapped at the meat within an hour of the time of application of the cocaine to its nostrils.

The eighth animal snapped at the meat a little more than half an hour after applying the cocaine to its nostrils.

Two other animals that were tried in the same way snapped at the meat within half an hour after being returned to the water.

Four animals in which a wad of raw cotton saturated with 5% cocaine was held in the mouth for three full minutes, were all thrown into such a state of coma that they were thought to be dead; one of these recovered in about an hour; the others required two hours to regain their usual activity.

Of two other animals in which the cotton with the 5% cocaine was kept in the mouth for one-half minute, one refused meat for the rest of the afternoon, about an hour, while the other snapped at the meat in less than half an hour.

While these experiments are not altogether satisfactory, it will be noticed that the cocaine introduced into the mouth inhibited in every case except one, the feeding reaction for an hour or more, and the shorter time in the exceptional case may have been due to the fact that the cotton was not kept in the animal's mouth long enough.

Whether the inhibition was due to the effect of the cocaine upon the nerves, or whether it was merely due to the general effect upon the animal as a whole, it is difficult to say, but the serious state into which several of the animals were thrown by too prolonged action of the cocaine, as described above, would seem to point to some general disturbance of the system that might incidentally inhibit the feeding reaction.

The application of the cocaine to the nostrils caused, as might be expected, a much more temporary inhibition of the feeding response, the failure to react to meat seldom lasting for more than half an hour, and in many cases the reaction was delayed an even shorter time. Just how much of the cocaine solution would penetrate the nasal chamber through the tiny external nares, it is impossible to determine, possibly very little.

Judging from the relative lengths of the inhibited periods, it would seem that either the animal scents its food more through the mouth than the nostrils, or that more of the cocaine found entrance into the nasal chamber from the cotton held in the mouth than entered through the nostrils when they were painted with the solution. Later experiments indicate that the latter is the case. This is assuming that in both cases the inhibition was nervous rather than a general disturbance of the system. The next experiment supports the view that the inhibition is due to the effect of cocaine upon the nerves rather than a general depression of the system.

Experiment 11. In this experiment five animals were used, and all were treated in exactly the same way. They were removed from the water and a wad of raw cotton wet with 5% cocaine was held in the mouth for half a minute, then the nostrils were painted with the same solution of cocaine for one to two minutes. When returned to the water all of the animals were more or less sluggish, four of them only slightly so, while the fifth was decidedly sluggish. After the cocaine treatment, the animals were subjected to a jet of acetic acid of a strength that in normal animals would cause quick and generally violent reaction, the jet being, of course, directed upon the anterior region of the head. Since four of the five animals reacted in practically the same manner, animal number 4 will be described as a type.

At 8.25 A. M., about five minutes after being returned to the water, the animal failed to respond to an entire pipette full of .25% acetic acid that was slowly squirted against its head. At 8.55 A. M. it responded very slowly to .5% acetic acid similarly used. At 9.10 A. M. it responded fairly quickly to .5% acid, but still much more slowly than normal. At 9.45 and 10.45 A. M. the response was still about the same as at 9. 10 A. M. At 5. 00 P. M. of the same day the response to the .5% acetic acid was as quick as in the normal animal, and the same was true at 10.30 A. M. the next day.

It may be possible, then, that the cocaine used in this way inhibited the chemical sense in the nasal and oral mucous membranes for a couple of hours, and during this time the slow and feeble reactions were merely those due to the stimulation of the skin over the head, which was little more sensitive than that of the rest of the body.

Experiment 12. To further test the importance of smell, using that term in the usual sense, in securing food, four animals were selected. It was found by dissecting preserved specimens that, by inserting the points of a fine pair of scissors into the two posterior nares, and cutting the intervening tissues, both olfactory nerves could be sectioned with one quick cut. With the four selected animals this was done, after administering just enough ether to keep them from struggling.

In three of the animals there was apparently no bleeding, but in the fourth the scissors failed to work properly, and there was considerable loss of blood. This animal was somewhat slower in recovering its activity and was found dead in the aquarium at the end of the second day. So far as observed, however, its reaction to food and acid were about the same as in the other three. These three recovered from the ether in a few minutes and the morning after the operation they were as active as ever, and gave no indication of being any the worse for the operation. Once or twice a day for more than a week they were tested with a bit of raw meat, but in no case attempted to seize it. Two of the animals paid no attention whatever· to the meat, while the third, on two or three occasions, followed the meat (and also a piece of filter paper) without snapping at it. Juice from raw meat and from earthworms, described above caused no reaction whatever, though samples of both caused the snapping response in normal animals.

After having been without food for about two weeks the animals, stimulated by extreme hunger, began to snap at meat or filter paper that was moved near them. If permitted to do so they would swallow the filter paper as readily as the meat. They would not seize either meat or paper unless it was in motion.

While it is hard to understand why, if sight be the sense used, cutting the olfactory nerves should make an animal less apt to *follow* a bit of meat or a tasteless piece of paper, the absolute refusal of these animals to eat, after severance of the olfactory nerves, seems to show that the olfactory sense is the one mainly used by Diemyctylus in recognizing food.

For two or three days after the operation, the response to a .5% solution of acetic acid on the head was slower than in the normal animals; but after that time the negative reaction was apparently as quick and as decided as in animals with uncut olfactory nerves. This would seem to indicate that the greater sensitiveness of the head region over other parts of the body to chemical stimulus, while it may be due merely to a greater sensitiveness of the skin of that region, is probably due to the sensitiveness of the nasal and oral mucous membranes, and not to any olfactory sense proper.

SUMMARY

1. Diemyctylus uses both sight and an olfactory sense in securing food, though the latter is apparently the more important.

2. The normal animal will follow and sometimes seize small bits of cotton or cloth of various colors, but will seldom swallow them; and when such objects are swallowed they are later disgorged. When fatigued with these objects so that it will no longer snap at them the animal will almost always seize and swallow bits of raw meat or earthworm.

3. There seems to be no difference in reaction between bits of raw meat and earthworms.

4. There is about the same response to filtered meat juice (both raw and cooked) as to the filtered juice from crushed earthworms. The response consists in snapping at and seizing the end of the pipette from which the juice is coming; also in snapping and searching motions after withdrawal of the pipette.

5. With the exception of the head all parts of the body seem to be equally sensitive to chemical stimulation. The head is much more sensitive than the rest of the body, even after the olfactory nerves are cut.

6. There is no definite reaction to a strong solution of sugar.

7. To a saturated solution of quinine a fairly strong negative reaction is obtained when the head is stimulated, but no reaction is obtained from other regions.

8. Negative reactions to a 1% sodium chloride solution are obtained from some animals when used upon the head. No reactions are obtained from stimulation of other regions with a solution more dilute than 4%.

9. 2% potassium hydroxide solutions cause quick and extremely violent reactions upon both head and body. The most dilute solution that causes reaction when ejected against the head is .0078125%. The reactions to this reagent are more violent than to any other chemical tried of equal percentage.

10. Negative reactions to acids are also very marked, and take

place with high dilutions. One part of acetic acid in about forty-two thousand parts of distilled water give weak reaction upon the head.

11. 1% ethyl alcohol is the weakest grade that causes negative reactions. The reactions to higher grades are slower than would be expected from the stinging sensation produced by these grades upon the human mucous membranes.

12. Cocaine, even up to 10%, has no apparent effect upon the skin of Diemyctylus. A 5% solution of cocaine applied to the nasal and oral mucous membranes temporarily inhibits the feeding reaction, and diminishes the sensitiveness to acid solutions squirted upon the head. This result may be due to the general effect of the cocaine upon the system rather than upon the nerve endings of the nose and mouth.

13. Animals in which both olfactory nerves are cut refuse, for two weeks, to eat meat or earthworms, or to pay any attention to the juices from these; but after recovering from the operation the response to .5% acetic acid is apparently unchanged.

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DISCUSSION OF CERTAIN QUESTIONS RAISED BY PARKER IN A REVIEW OF "LIGHT AND THE BEHAVIOR OF ORGANISMS"

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In this journal, Vol. 1, pp. 461-464, there appeared a review by G. H. Parker of the book entitled "Light and the Behavior of Organisms." Certain statements in this review seem to indicate that the position of the author of the book, the present writer, was not grasped, other statements show clearly that the author of the review takes exception to the interpretation of various phenomena as described by the author of the book. The questions at issue may be conveniently taken up *seriatim.*

1. Parker says, p. 461, "The facts, new and old, are marshalled with great consistency and uniformity in support of Jennings's well-known views on animal orientation, and this method of presentation is often so vigorously pursued as to give the reader the impression of special pleading rather than sound judgment. Many of the cases chosen to illustrate the 'trial and error' method of orientation must strike the impartial reader as equally good examples for the support of the 'tropism' theory." I do not understand how my reviewer came to the conclusion that cases were chosen to illustrate the "trial and error theory" or any other theory of orientation. I reviewed practically all previous work on orientation and thoroughly investigated the subject myself, studying the process in representative species of plants and of all of the different classes of animals. The methods of orientation in all are described in detail regardless of what theories they fit. Judging from Parker's statement one might conclude that all had been discarded except those which fit preconceived notions. Whether or not "sound judgment" was exercised in dealing with the subjects treated is a question which I shall leave in the hands of my readers.

2. Parker implies repeatedly in his review that the methods of orientation as described in my book support "the tropism theory." But unfortunately he fails to state which one of the fifteen[1] different tropism theories presented in my book, pp.

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[1] There are now seventeen, two new ones having recently appeared.

53-55, he considers as "the tropism theory," and without such a statement his criticism is meaningless, for every method of orientation described is in accord with one or another of the fifteen theories presented. They are practically all tropisms in one sense or another and I have nowhere even so much as intimated that they are not. I have however pointed out in many instances that various methods of orientation do not support certain prominent tropism theories. I am greatly disappointed that my critic has used this term in such a loose way after all the time and energy I spent in trying to show what endless confusion has been caused by just such carelessness.

In answer to two letters addressed to Professor Parker requesting first, a definition of the term tropism and second an explanation of its meaning as used in his criticism he says in his first reply: "I frankly confess that I cannot give a clear definition of the tropism theory or the theory of trial and error"; and in his second: "The gist of the tropism idea as it lies in my mind is that it is an orientation reaction dependent upon unequal stimulation of two symmetrical parts of the organism's body."[2]

Since Parker admits that he cannot define clearly either the tropism or the trial and error theory it is not surprising that he concludes, as quoted above, that certain cases which illustrate the one serve equally well to support the other. Hazy definitions invariably lead to confused thinking. I may add however that I have had no difficulty in understanding what Jennings means by trial and error, and I assume that this is what my critic implies by the "theory of trial and error."

3. In the process of readjustment to a change in the direction of illumination, Stentor may either stop and turn abruptly or it may turn gradually. In the gradual turning from the source of light Parker says we have a "good instance of tropic response." This conclusion like the one referred to above is not surprising on the basis of a tropism theory that cannot be defined clearly. But let us apply the definition Parker furnished in his second letter and see what ground there is for it. By referring to this definition quoted above it will be seen at once that it can be applied strictly only to bilaterally or radially

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[2] In a later communication Parker requests that it be made clear that in his first reply he refers to a definition which would be acceptable to a majority of those using the term, and that in his second reply he gives a personal definition.

symmetrical organisms since only such have "two symmetrical parts." Stentor is neither radially nor bilaterally symmetrical and consequently strictly speaking none of its reactions can fit Parker's definition of tropisms. Even if we interpret this definition very liberally and substitute opposite sides in place of symmetrical parts there is no evidence that the gradual turning of Stentor from the light is tropic as Parker maintains, for it responds only when, in the process of turning the light intensity on the oral side is increased, not when other surfaces become more strongly illuminated. This I have fully described in the text as well as in the legend, Fig. 14, p. 117. In spite of much time and energy spent I was unable to obtain any evidence whatsoever indicating that any reactions in Stentor are dependent upon unequal stimulation of opposite sides, pp. 114-115. It is consequently evident that as far as my experiments and observations are concerned there is no evidence whatever indicating that any features in the process of orientation in Stentor are "tropic" in accord with Parker's definition of this term.

4. It is maintained by my critic that I have not "made clear how an organism, after it has once become oriented, can continue to move in a straight line without involving the essential elements of the tropism theory." I assume that in this connection he means by "essential elements of the tropism theory" reactions due to equal "stimulation of symmetrical parts of the body. If my description of the process of orientation in Euglena, pp. 92-97, Stentor, pp. 114-119, fly larvae, pp. 189-193, and a number of other organisms is understood it will be clear without further elucidation how organisms can take a fairly straight course without equal stimulation of symmetrical parts. (The idea that organisms move in a "straight line" is more imaginary than real.) I have stated my position regarding this matter referring to Euglena (p. 230) briefly but clearly as follows: "The reactions caused by changes of intensity result in directing the organisms toward various points of the compass. As soon as they reach a position in which the rotation on the long axis no longer causes a change of intensity on the sensitive region there is no longer any cause for turning; they therefore continue in this direction."

According to this explanation the organism retains a given direction of locomotion not because of continuous action of light, but because it tends to move in a direct course in the absence of external stimuli. Thus we find that earthworms, e. g., oriented in light, continue in a given direction for some distance after the light is entirely cut off. The most striking demonstration of this is seen in fire-flies in which the orienting stimulus consists of a flash of lght. Orientation and movement toward the place where the flash occurred takes place in darkness. Here then there is no possibility of light acting continuously in directing the animal on its course. Why should it be necessary in other forms as Parker assumes?

5. "But the chief general defect of the volume," says Parker, "is one that has been inherited from earlier students in this field of work, and consists in the attempt to apply the trial and error method of orientation to the movements of many of the higher invertebrates, such as the earthworm, fly larvae, etc., to the exclusion of the tropism idea." He then singles out the reaction of the earthworm for special criticism with reference to this point. On the basis of his own definition previously quoted have we in reality attempted to apply "the trial and error theory . . . . to the exclusion of the tropism idea?" On page 200 we find the following sentence: "If the [earthworm] however is rather sluggish so that there is little lateral movement of the anterior end it turns from the source of light with very few exceptions" (very few trials). On the same page there is a table which shows that out of 90 exposures to unilateral illumination the specimens used turned directly from the light 80 times and toward it only 10 times (scarcely any indication of trial). And again at the bottom of the page I have made the following statement: "In a few other sluggish specimens the exposure to unilateral illumination was not made until after they had come to rest in light of 15 candle meters. Under such conditions the animals did not react at all until a few moments after the exposure, then they very slowly extended and turned the interior end from the source of light every time. . . . . There was no evidence of even the slightest preliminary turning toward the source of light" (no trials whatever). After stating that trial reactions may function in the orientation of the earthworm I wrote, p. 204: "This does not indicate that constant light cannot also produce orienting stimulations." That is, it does not indicate that there are no tropisms even in accord

with Loeb's definition. And finally in the summary, p. 205, we find the following: "Under certain conditions, if one side is illuminated, they [earthworms] always turn toward the shaded side without preliminary movements and therefore orient directly" (no trials). Can it be possible that my critic actually read the section of my book containing the quotation given above and still came to the conclusion that I have "attempted to apply the trial and error theory . . . to the exclusion of the tropism idea," assuming that he meant by the "tropism idea" merely orientation without trial movements. The truth of the matter is that no attempt was made to apply any theory whatsoever, but there was a very serious attempt made to describe precisely how orientation occurs in all of the different forms studied. In the fly larvae it is true I found no evidence of direct orientation, no reactions in accord with Parker's definition of tropism. But by merely glancing over the general summary, pp. 228-235, it will be seen that according to my conclusions, direct orientation, i. e. orientation without trial is found in coelenterates, earthworms, planaria, mollusks, arthropods, and vertebrates. Note the following statement found on page 233: "mollusks, arthropods and vertebrates all orient directly. There is little evidence of preliminary trial movements in this process in these forms." Do not these statements show that if there was an attempt made to apply trial reactions to the exclusion of other factors in explaining orientation the attempt must have been a miserable failure?

Orientation is unquestionably direct in many instances, i. e., without trial and in accord with Parker's definition of tropisms, provided he includes under the phrase "unequal stimulation of symmetrical parts," cases in which the stimulus may be confined to either side, it being zero on the opposite side; which would in reality mean that orientation may be due to a differential response to localised stimulation. If however he means to imply in his definition that both sides of the organism are continuously unequally stimulated during the process of orientation and continuously equally stimulated after orientation in accord with Loeb's last theory of tropisms, I have no conclusive evidence in support of his "tropism idea."

6. In discussing the efficiency of trial in the process of orientation Parker bases his argument upon a hypothetical case. Assum-

ing that an earthworm becomes oriented after making five preliminary trial movements, he says, p. 463: "In this total act the significant part so far as ultimate orientation is concerned is not the making of the five trial and error movements but the final 'selection' of the best one as a line to be followed in the real locomotor act. Now it is difficult for the reviewer to see how this 'selection' can be accomplished without involving just that feature of symmetrical stimulation which is the essential part of the tropism idea. The worm presumably moves finally in the direction of that trial which through the stimulation of symmetrical points on its body has been found to be most favorable for orientation. Without complicating this problem by raising the question as to the means by which the worm could retain impressions of the five trial and error movements preparatory to following up one of them, it must be evident that these movements in their effect on orientation are quite subordinate to symmetrical stimulation, in fact, not essentially involved in orientation at all."

What necessity is there for assuming "symmetrical stimulation" and "retention of impressions" of preceding movements in the process of orientation by trial? Contrary to Parker's implication I did not emphasise trial as a factor of exceptional importance in the orientation of the earthworm and I referred to it only very briefly. But I did discuss this question rather fully in my description of the process of orientation in fly larvae, pp. 189-196. Careful reading of this description will convince almost any one that there is no necessity whatever of assuming "symmetrical stimulation" or "retention of impressions" to account for orientation by trial reactions. Not only is this true in the case of fly larvae but there is good experimental evidence for opposing especially the former assumption. This evidence is found on pages 194 and 195 with the following conclusion: "The orienting reactions [of fly larvae] could readily be explained by assuming the area sensitive to light to be restricted to a small mass of substance located in the middle of the very tip of the anterior end."

But my reviewer is not the only one who seems to have difficulty in understanding how orientation by trial could occur without the activity of psychic factors, to say nothing about mystical factors of various sorts. Witness e. g. the statements of Bohn and Loeb in all of their recent papers referring to this subject. And this in spite of the fact that in the numerous descriptions of orientation by this method given by Jennings and myself no such factors whatever are involved or implied.

The main difficulty these authors have in grasping the subject lies in their assumption that locomotion is dependent upon the same factors which regulate the direction of locomotion, an assumption for which, as far as I am aware, there is absolutely no evidence.

Let us attempt to clear up these matters by means of an illustration: Suppose an active blind fish be put into an enclosure with a small opening at one side leading into a long narrow straight passage. The fish swims about, strikes the wall of the enclosure, turns and takes another course, strikes the wall again, then proceeds in a different direction, etc., until finally it strikes the opening and enters the narrow passage. The fish has become oriented and if there is nothing to turn it out of its course it may proceed through the passage without further stimulation, but if it is turned out of its course it strikes the wall of the passage, turns and soon becomes oriented again. Surely no one will maintain that it is necessary to assume " symmetrical stimulation " and " retention of impressions " of the various movements in order to account for the orientation of the fish. And what basis is there for the assumption of psychic factors! It would indeed be a comparatively simple task to construct a machine with wheels and springs and levers so arranged that after striking an obstacle it would back off and take a different course, and if put into an enclosure similar to that assumed in case of the fish it would finally strike the opening and become oriented. As far as trial movements are involved in the orientation of fly larvae or earthworms the process is, in principle, precisely like this. The organism just like the hypothetical machine or the blind fish in the enclosure continues to turn in different directions assuming different axial positions until the stimulus which causes the turning ceases. And the same principle is involved in every account of orientation by trial in the lower organisms that has been given either by Professor Jennings or myself. Note, e. g., the following statement from Jennings quoted in my book, p. 47: "[The so called trial and error method of orientation] consists in successively ' trying ' not only different directions of locomotion, but also different positions of the body axis. As soon therefore as a position is reached in which the disturbance causing the reaction no longer exists, the reaction of course stops; the animal therefore retains this axial position."

I am unable to understand how my critic could have read this statement and the account I have given of orientation by the trial method in fly larvae and various other forms and still maintain that "symmetrical stimulation" and "retention of impressions" is necessary to account for the process observed and described.

7. Professor Parker implies that there is serious confusion in the sequence of authors' names throughout the entire volume. He writes, p. 463: "It is difficult to understand the ground for the sequence of authors' names such as occurs in the categorical statement made on pages 265 and 266, where, contrary to the common practice, the order of dates is not followed. This looseness at times comes to be really misleading where, for instance on page 52, Verworn precedes Loeb with dates 1886 and 1887, while in the bibliography Loeb's earliest reference is given with the date 1888, and Verworn's with that of 1889." The looseness referred to in this statement is more imaginary than real as any one interested can ascertain for himself. As a matter of fact the criticism would not call for a reply if it did not bring up the question of priority between Loeb and Verworn.

The sentence containing the dates to which Parker refers in this connection reads as follows: "The study of animal behavior from the physico-chemical point of view was first taken up by Verworn and Loeb in 1886 and 1887." The dates found here clearly have nothing to do with dates of publications recorded in the bibliography, and I can see no reason why they should be misleading, especially since, in referring to the investigations of Verworn on the activities of protozoa, I stated, p. 6, that "these were taken up in 1886, two years before Loeb's first preliminary note on the reactions of animals appeared." I did not go into the matter further because I was not primarily interested in the question of priority concerning the authors in question. In all probability each of them began work on animal behavior from a purely objective point of view independent of the other. The facts concerning this matter are as follows. In 1889 Verworn published a paper entitled Psycho-physiologische

Protisten Studien" covering 218 pages. In this he describes in detail the reactions of a number of animals and discusses them from a physico-chemical point of view. In the preface it is stated that he began the work in 1886 just as I stated in my account.

Loeb's first paper dealing with behavior appeared in 1888, under the title "Die Orientieung der Thiere gegen das Light. (Thierischer Heliotropismus)." It is a sort of preliminary report consisting of but five pages published in the proceedings of a medical society at Würzburg. A much longer paper with a similar title appeared later in pamphlet form. This pamphlet is dated 1890 indicating that it was published during the year following the date of publication of Verworn's paper. Loeb, however, usually gives the date 1889 in referring to this paper (see Studies in General Physiology and Dynamics of Living Matter) thus intimating that it was not preceded by Verworn's paper. Why he gives an earlier date than that found on the pamphlet I am unable to say.

These are all of the important facts at my command concerning the dates of the earlier works of these authors. Those interested in the question of priority may use them as they see fit in drawing their conclusions. However in discussing priority of work on the behavior of organisms from the mechanical point of view the brilliant investigations of Engelmann beginning as early as 1869 must not be overlooked.

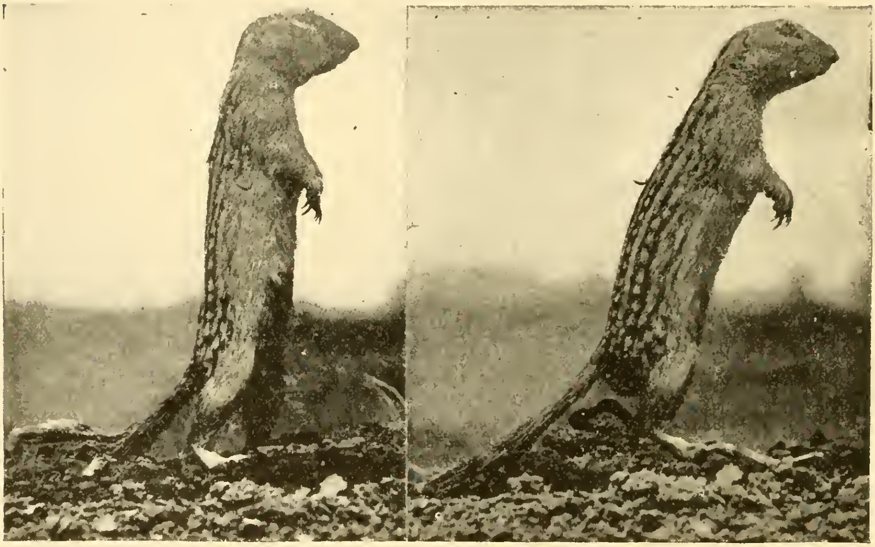
NOTES

PECULIAR ACTIONS OF A STRIPED GROUND SQUIRREL

ROBERT B. ROCKWELL
Denver, Colorado

Glimpses of the inner nature of wild creatures are encountered by observers at the most unexpected times and places, and such an experience fell to the writer's good fortune early last June.

Some fifteen miles northwest of Denver, Colorado, is a small lake leased by a number of Denver men for duck shooting. The so-called "club house" is a frame structure of one room, which serves the manifold purpose of kitchen, sleeping room, store room, etc. It is occupied but one night during the week for only a few weeks in the spring and fall and is overrun with house mice. The shooting season closed April 15th, and the house was not again occupied until the night of June 3d. On this evening the writer, with a companion, reached the cabin some time after dark, and preparations were made to retire.



In the corner of the cabin was a pile of three folding spring cots, and on top of these was a camp mattress. The cots and mattress were taken out of doors and set up (for we were outdoor sleepers) and the writer promptly turned in. I had no sooner become quiet than I heard a chorus of tiny squeaks coming from inside the mattress, and an examination revealed a warm nest in the padding containing eight tiny, naked, blind creatures whose eyes were not yet opened. Supposing them to be young mice they were ruthlessly disposed of, as mice usually are, and I prepared to resume my slumbers.

I was aroused a few minutes later by my friend (who had not yet retired) calling me softly to hurry into the cabin, and once there a sight met my eyes that I will long remember.

Under where the cots had been piled was a knothole in the floor; this, however was not directly under the position of the nest, which was rather toward one side of the mattress but more to the other side of the space which had been covered by the cots and both were well toward one end of the latter. Emerging from this hole was the mother of the little ones we had just assassinated, but instead of a mouse, it proved to be a large, handsome Pale Striped Ground Squirrel (*Citellus tridecemlineatus pallidus*). As this species is, to the best of my knowledge, strictly diurnal, and the time was between 10 and 11 P. M., it seems very probable that the animal had been frightened from the nest by our entrance and was now returning.

Upon reaching the floor she seemed greatly confused, but after some hesitation she ran to the place from which she had probably been in the habit of climbing up the frames of the cots to the nest. Here she stood upon her hind legs and frantically pawed the air, apparently trying to locate the missing frames. Then she ran back to the hole and repeated the entire performance. She would stand straight up on her hind legs, as straight as a man could stand, and after turning round and round, would fall backward, only to jump up and run about in various sized circles in a vain endeavor to locate her missing family. This performance was continued without a pause for fully fifteen minutes, during which time her excitement and confusion visibly increased, and although we were both within arm's length of her and spoke frequently in an undertone, she did not pay the slightest attention to us. Several times she jumped high in the air from a standing position and falling to clutch the accustomed frame would fall backward, after which she would spin around repeatedly much after the fashion of a wounded creature. She did this always from practically the same spot. During the latter part of the performance she continually gave utterance to low cries and moans, and her entire manner gave the plainest evidence of grief and excitement. In all my experience with wild creatures, I have never witnessed a more keenly pathetic scene.

Finally, after we had satisfied ourselves that further observations would reveal nothing new, we decided to capture her and photograph her the following morning (it was by this time almost midnight), and here we received a great surprise, for, although she eluded our hands and in doing so exhibited wonderful strength for so small an animal, she did so in a half-hearted manner, and although the knothole was within a few feet, she did not attempt to escape by that means except once, and even then she was back going through her queer performance within a minute.

Previous experience with these little creatures had taught me a wholesome respect for their sharp teeth, so we approached her with heavy gloves, but even when we held her tightly in our hands she exhibited neither fear or anger, and instead of biting fiercely as is usual with this species, she simply nibbled feebly at the gloved finger much as a house cat might scratch at a door it wanted opened.

After capturing the animal three times and allowing her to escape each time for fear of injuring her in her struggles, she was finally placed in a box and we retired for the night. The next morning we made a narrow box with glass sides and no bottom and prepared to make a series of studies. She was somewhat wilder in the sunlight and became quite panic stricken when first put into the glass box, the glass sides being altogether beyond her comprehension; but after a few minutes she quieted down and a highly interesting series of photographs was the result, one of which published herewith shows the erect position above described.

After half a dozen exposures had been made the captive was liberated and promptly scampered into the nearest burrow, and since that time we seldom visited the "club" during the long summer and fall without being greeted by her sharp, clear whistle, though she took particular pains to keep far away from our inquisitive fingers.

COMMENTS ON MR. ROCKWELL'S OBSERVATIONS

By EDWARD R. WARREN
Colorado Springs, Colorado

The preceding notes were written by Mr. Rockwell at my request after he had told me about the occurrence in conversation, as it seemed so interesting as to be worthy of record.

The animal seems to have been unable to realize that the cots, which had not only supported the mattress in which the nest was, but also covered the hole in the floor by which she had entered the room, were missing. One would think she would have noticed their absence at once, as heretofore they had been there when she went to and fro. It is possible that being a diurnal animal, her confusion may have been due to being disturbed in the night, and that she was even unable to see well by such light as my friend may have had there, though this last would not account for her actions. It is an interesting speculation as to whether she would have acted as she did if it had been daylight.

DELAYED IMITATION IN A CAT

EDWARD R. WARREN
Colorado Springs, Colorado

The two cats whose behavior is described in the following notes were own brothers, of the same litter, born in July, 1898, on a ranch near Crested Butte, Gunnison county, Colorado. As they were named, I shall refer to them hereafter by their names, "Tom" and "Snowball." The former was a large-boned yellow and white cat, never very fleshy, the latter a chunky, black animal, apparently smaller, but weighing nearly as much as Tom. Both were castrated when eight or nine months old. The winter of 1898–9 spent at the ranch with the son of the owner, with the cats for company. Just how it came about I do not remember, but Tom learned the trick of coming up on my lap when I was sitting in a chair, thence to my shoulder and on my outstretched arm and taking meat held on a fork. Sometimes he would do it if I was standing up. Snowball would never do this.

I left the ranch the spring of 1899, but remained in the vicinity and frequently visited there until late in the fall of 1902. Whenever there at a meal, and that was quite often, I usually fed Tom in this way for amusement, but none of the members of my friend's family ever troubled themselves with it. Snowball was invariably on hand at mealtimes, and his appetite was as good as his brother's, but, though I used to talk to him and coax him, he would never come up after the meat. His "stunt" was to pull the meat from the fork with his paw and convey it to his mouth that way, but only when he could reach the fork from the floor.

I was absent the winter of 1902–3, returning for a short time in July, 1903, and Tom came up for the meat readily at the first offer, and Snowball, as previously, refused. From July, 1903, to October, 1905, I did not see the cats, but the last mentioned date made another visit, and as in 1903, Tom showed he had not forgotten, and scrambled up at once when invited, while Snowball did not.

Then came a still longer interval, until January, 1909, before

I returned for a visit. The cats were active and lively, and it will be noted, were in their eleventh year. I asked my friends it anyone had fed Tom in my way since I had been there last, and they said no one had done so. So I had much curiosity to see if he would remember, as it was now over three years since I was there last. Sitting in a chair and taking a bit of meat on a fork and holding it out at arm's length (out to one side, not in front), I spoke to Tom and told him to come up if he wanted it. He looked at me and at the meat, and seemed puzzled; I continued talking to him and then he apparently remembered, but imperfectly or doubtfully, for, instead of coming up with a rush as had been his former habit, this first time he climbed up rather deliberately and hesitatingly and took the meat. But only the first time did he hesitate; after that he wasted no time. Snowball was, of course, present while this was going on. Unfortunately I kept no notes of the matter, so cannot say if it was the first time or a meal or a day or two after that the surprising thing took place. Tom had taken a piece of meat from the fork and jumped to the floor to eat it and I was holding out more, talking to Snowball, who was begging for it, telling him it was his for the taking, and apparently he decided he could do what his brother did, for suddenly he came with a scramble to my shoulder and secured the tidbit.

To say I was surprised puts it mildly, for I looked for nothing of the sort after all those years and was merely teasing him as I always did. I had always given him his share in his own way. I remained nearly two weeks, and during the remainder of the time Snowball vied with Tom in taking the meat, often there was a race up on my lap for it. There was another cat at the ranch (and it was not a cat ranch!) a half brother of these two, nine months younger, "Sunflower" by name, or "Sunny," for short, a large yellow animal. His appetite was as good as the others, but he would not climb for his food, so went without to some extent. I do not wish anyone to infer that feeding from the fork was the only way those cats were fed, for they had plenty to eat besides, and were out about much after mice. The feeding with the fork was simply done for amusement.

It is certainly very strange that Snowball should have waited so long before learning his brother's trick or imitating him, however it may be best expressed, when he had seen it done so many times before. If he had not seen it frequently done, it would not be so odd, but it can be said, with little or no exaggeration that he was present every time Tom got a bit of meat in that fashion, and, moreover, as I have said before, I always talked to him and coaxed him to take the same way also. It was not due to any stupidity on his part, I think, for he always seemed as bright, smart and active as his brother, in fact he seemed quicker in some ways. I make no pretense of being a psychologist, and will leave it to those who are, to explain this behavior. I have stated the facts as clearly and correctly as I could.

PSYCHOLOGICAL RESULTS IN REACTIONS TO TONE BEFORE AND AFTER EXTIRPATION OF THE TEMPORAL LOBES.

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From the laboratory of Professor L. Jacobsohn, Berlin, Germany.

A German court dog, Louisa, was trained to take meat at a low tone (eating tone of Kalisher) and refrain from taking it at a high one. Then his left temporal lobe was removed by Prof. L. Jacobsohn in Berlin. After three days' rest, the same training was continued, and when good response was re-established, the right temporal lobe was removed by him. Training was resumed by me.

I give here merely an epitome of the psychological results of the training and operations. The normal dog under my training took about fourteen days to learn to react correctly and eat at the low tone and refrain at the high tone. At first he grabbed at the food indiscriminately, tone or no tone, then (after severe punishment), when the high tone was sounded, gradually learned to refrain at the high tone for fear of punishment. After two weeks of such discipline, one could easily see that the dog had some clear idea that he must refrain at one tone and eat at the other. During a month of such training, gradual improvement resulted so that reaction was quicker, more exact; more being mistakes were corrected. I found at first he was inclined to react to motion rather than sound, and watched me closely for the motion accompanying the low tone, and would react to that. Too little food resulted in an eagerness to eat, and made many failures, and too much food brought a refusal to act at all. When this trick was well learned, he was operated on as above, and after three days' rest, training was resumed by me; and at the first test, he reacted correctly and ate at the low tone. But reaction came now decidedly slower than before, and never up to the second operation regained quite the exactness and quickness that the dog showed before any operation was performed. A gradual increase was, however, noticed in these qualities of execution. After many days had passed, during which best results possible were obtained, the right temporal lobe was removed, and the dog, without any temporal lobes, was trained again as before. And now appears the result that surprises the student of brain localization.

After a period of training, lasting two weeks with no sure response to sound—very gradually the ability to differentiate the high tone from the low one returned. Slowness and almost an ataxia in the head movement with an occasional false act characterized the whole period of reactions after the second operation. But it was plain, as was testified by some eminent scientists who came to see the dog in the laboratory in Berlin, by the members of the German Neurological Society before whom I demonstrated him, and also in this country, by the Boston Neurological Society, and other scientists, including Prof. Yerkes, who saw the dog at my home in Boston—it was, I say, plain to all these men that the dog reacted to a sound stimulus and differentiated two tones when both temporal lobes had been extirpated. Prof. Yerkes pronounced him acutely sensitive to sound, and suggested that I try to teach him a set of new associations, so I tried making him get up to a chair placed before him to reach the meat, rather than bob his head downward for the meat on a block below him. This met with success for even in only a few trials, the dog shot his head upward for the meat at the eating tone, when before he had dropped it directly downward. This too, when a blast of air was introduced to eliminate smell as a guiding agent in directing the reactions.

For a more exhaustive report of methods, technique, macroscopical and microscopical findings, the reader is referred to the report of a paper read in Berlin before the Gesellschaft für Psychiatrie und Neurologie in the Neurologisches Centralblatt, November 13, 1910, and to the Journal of Nervous and Mental Disease.

The autopsy findings, i.e., the macroscopic appearance of the area where the brain substance was extirpated, show a total extirpation of both temporal lobes. (Only the microscopic findings to appear later can add further data.)

These results lend weight to Kalischer's findings, although we offer a different interpretation. Kalischer claimed that the association process of tone differentiation was a mere reflex located in the basal ganglia and like other mere reflexes, as the Achilles reflex and the knee jerk (exclusive of its cerebral element), had only a peripheral activity consisting in an afferent impulse with a ganglionic transfer of motion into a motor impulse passing over an efferent nerve with absolutely no cortical relation whatever. Rothman, failing to obtain any positive results in tone differentiation, concluded he had extirpated the cortical area subserving that function. The conclusions of Prof. Jacobsohn and the writer place the functions in an entirely different location from either the basal ganglia or the extirpated temporal lobe.

We locate the differentiation of tone in the cortex ex-temporal. My reasons for this cortical localization are the presence of psychological elements in the execution of the act which differentiates the execution from that of a mere reflex. These psychological elements are *purposefulness*, *slowness*, *inhibition*, and the need always to *relearn* the execution of the constellation of inter-related perceptions and executions.

To take these up briefly in detail:

(1) A mere reflex is always quick—blitzartig as the Germans say—it shoots the determined act off "too quick for thought." The eye reflex, various protective reflexes, and in the neurological field the corneal, cremaster, and the knee jerk, recall the quickness of the motor response to stimuli. In contradistinction, the act of Louisa to tone had a noticeable ever present *slowness* about it. This alone to me would be pathogneumonic of a premeditated cortical act.

(2) Then *inhibition* is another quality that is foreign to the mere reflex. The constant waiting and watchfulness to listen and differentiate the tones and the persistent prevention of motor action at the wrong time shows the inhibition in its cerebral sense was a pretty constant factor in the behavior of my dog Louisa.

(3) All know that mere reflexes are without sustained purposes and surely the eating act to tone stimulus had an ever present *purpose*.

(4) Mere reflexes are never learned, never have to be *relearned*, never are forgotten and have to be recalled to memory. All of these elements—qualities of cortical function—were pres-

ent at one time or another during the one and a half years of training to which I subjected my dog.

Therefore I concluded that the location of the act was in the cortex somewhere outside of the extirpated temporal lobe.

To sum up: A dog may be trained to differentiate two tones; and after extirpation of both temporal lobes, has ability to react differently to different tones, and can be taught new associations.

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NOTE ON THE NEGATIVE REACTION UNDER LIGHT-ADAPTATION IN THE PLANARIAN

EDWIN G. BORING

From the Cornell Laboratory for Comparative Psychology

One figure

It has been shown[1] that the planarian tends to become active under the influence of light and to come to rest in the shadow or dark,[2] a form of behavior that results most generally in bringing the animal into regions distant from the source of light. It has also been shown [3] that photic stimulation elicits a negative response; that is to say, the animal tends to turn from the source of light. This response is due, however, not to the direction of the rays of light as such, but to a tendency on the part of the animal to turn from the side of more intense illumination. A planarian will move along a narrow strip of shadow directly toward the source of light, if in so doing it avoids the areas of more intense illumination on either side of the strip.[4] The turning from the light in directive illumination has thus been explained as a negative response to a difference of intensity of light upon the two sides; in other words, the creature, being more strongly illuminated on the side toward the light than on the side away from the light, gives the negative reaction away from the light source. In explanation of this response Loeb [5] has applied the term *Unterschiedsempfindlichkeit*, as opposed to a heliotropism under which the animal responds to the direction of the light, independent of its intensity; and the most generally accepted explanation appears to be some form of differential sensitivity. This view seems, however, to have been accepted less upon positive experimental evidence than because of the inadequacy of the other theories.

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[1] Loeb, J. *Arch. f. d. ges. Physiol.*, *54*, 1893, p. 101; *56*, 1894, p. 255.

[2] This form of behavior has been called *negative photopathy* by Washburn, *Animal Mind*, 1908, pp. 167ff.; *photokinesis* by Englemann, *Arch. f. d. ges. Physiol.*, *30*, 1883, pp. 107ff. Walter later applied the term *photokinesis* to the behavior of planarians, *Jour. Exper. Zool.*, *5*, 1907, p. 49.

[3] Parker and Burnett. *Am. Jour. Physiol.*, *4*, 1900, pp. 373ff.

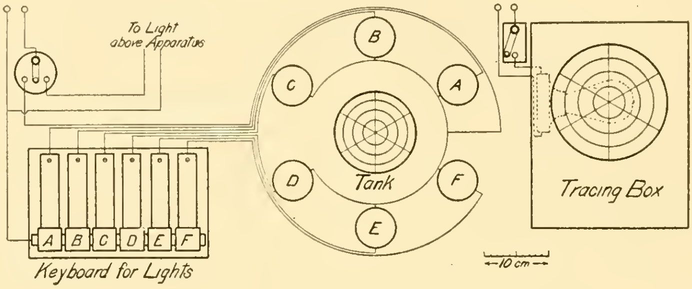
[4] Cole, L. J. *Jour. Comp. Neur. and Psych.*, *17*, 1907, p. 193.

[5] *Arch. f. d. ges. Physiol.*, *54*, 1893, p. 101.

In the course of experiments upon the modification of behavior in the flat-worm, the writer believes that he has observed behavior that argues directly in favor of a differential sensitivity; and it is the purpose of this note to describe the observations.

MATERIAL

The subjects used were specimens obtained from a stream near Ithaca, New York, and were partially identified as *Planaria torva*, although the identification was not absolutely positive.



APPARATUS

The apparatus was set up in a dark room. The essential features consisted of a circular glass tank, for which a crystallization dish was used, 12.5 cm. in diameter by 6 cm. high, and six 16 cp. electric lights (A, B, C, D, E, and F in the figure), arranged in a circle about the tank, 15 cm. from its center, and set at such a height that the centers of the bulbs were about 1 cm. above the level of the bottom of the tank. The lights were so arranged that they could be individually thrown in or out of circuit. They were controlled from a keyboard, which was constructed of strips of tin. The strips were insulated at the points of contact with the fingers, and so placed above a metal rod, that each strip, when pressed down, completed the circuit for a single lamp. One meter above the center of the tank was suspended a single 16 cp. light. This light and the system of lights below were controlled by a double-throw switch, in such a way that when the lower lights were in use the upper could not be, and *vice versa*. The tank was placed upon a sheet of paper, ruled with four concentric circles of diameters, 12.5, 9.0, 5.5, and 3.5 cm., respectively, and with three diameters, drawn 60 degrees apart. (See figure.) An enlarged duplicate of this ruling was pasted upon a sheet of glass, 23 by 33 cm., which formed the top of the tracing box. The tracing box was an ordinary wooden box, containing an electric light and so arranged that when a blank piece of paper was placed upon its glass cover the diagram upon its top could be seen through the paper. Since the diagram reproduced that beneath the tank, it was possible to trace quite accurately on a paper upon the box the movements of the planarian in the tank. The top of the box was about 15 cm. above the bottom of the tank, and the light coming through the paper and glass was small, so that the illumination in the box had practically no effect upon the illumination in the tank. When the tracing box alone was lighted, the observer was but just able to make out the contour of the tank.

METHOD

The procedure, in general, was as follows. A rested, but hungry, individual was removed from the dark and placed in about 1 cm. of water in the tank. Then, with only the normal illumination furnished by the light above the apparatus, its course for five minutes was traced as accurately as possible on the tracing box. The subject was then left in the dark for two minutes, after which one of the lower lights in the circle, opposite one side and slightly in front of the animal, was thrown in at the keyboard. As the subject was brought into activity by the light and turned away from it, another light was thrown on in place of the first one, so that the same side of the animal as before was lighted. Thus the planarian was kept constantly turning and turning, while the source of illumination, controlled from the keyboard, kept following it about. As the head is more sensitive to light than the rest of the body, it was found advisable to anticipate the turn, in order to keep the planarian away from the sides of the tank, by throwing on each light about 20 degrees before the long axis of the animal became perpendicular to the radius passing through the light. The light was then left on until the animal had passed 40 degrees beyond the radius. This procedure was possible only in slow, consistent movements; for when the subject turned rapidly it was possible only to follow it hurriedly with the light, often skipping a single lamp; and again, when the animal became "unruly," it was sometimes necessary to throw on two lights at once in order to keep it from the sides of the tank. However, this latter expedient was seldom resorted to and never continued for more than a few seconds at the longest. After the individual had been subjected to intense illumination from one side for about 30 minutes, it was left in the dark for 2 minutes, and then allowed to glide freely for 5 minutes with non-directive illumination from the light above,—an intensity of illumination approximately one-fiftieth that from one of the lower lights.

An attempt was made in many cases to supplement the observations described by a continuation of the series, alternating thirty-minute periods of right or left illumination with five-minute periods of non-directive illumination; but great difficulty was experienced in finding subjects which would remain active for these extended periods under violent stimulation. Many of the forty-four-minute series described were abandoned because the animals would not remain active. With two of the eight subjects reported upon here (B and E), however, longer series were obtained,— B remaining active, with only occasional mechanical stimulation, for five hours and twenty-two minutes. In this connection, it should be added that, although all the individuals were obtained from the same place, A, B, and E were from a lot taken at a different time from the others.

It was not found possible in all the experiments to keep the exact lengths of periods constant. Now and then, especially in the prolonged series upon individuals B and E, the animal would become sluggish and it would be necessary to terminate the trial before the thirty minutes had elapsed. The four minutes in the dark and the five minutes in the subdued light would, however, frequently refresh it enough to permit the starting of a new trial. In the two prolonged series, whenever the subjects became very sluggish, it was necessary to stimulate them to activity by a slight prod with a stiff hair. In such a case, a subject was prodded only after it had remained inactive for two minutes. This procedure was considered allowable, as the important factors--namely, the direction of turning and the continued exposure of one side to light—were not presumably affected thereby. Very rarely the subject would come to the side of the tank, in spite of the tendency of the light to turn it, and start to glide along the tank-wall. In these cases it was immediately removed by a section-lifter to the center of the tank.

GENERAL BEHAVIOR

The behavior of the animal under strong illumination from one side may be described as a continuous, steady glide, occasionally interrupted by "wigwag" movements; but in general, either by gradual or abrupt turns, it keeps its negative orientation to the light. At times, however, the animal suddenly moves toward the light, often rapidly making two or three complete turns in this direction before settling back to its previous course. The behavior of the flat-worm under normal illumination, following strong, directive illumination, is similar to that under intense light, except that the general direction of curvature is reversed and that quick, abrupt turning in the direction opposite to the principal one is less frequent.

DATA

In measuring the records of the paths traced by the planarians the following method was adopted. As the courses were exceedingly tortuous, they were considered as including no straight lines; that is to say, as always inclined either to the right or to the left. All points of inflexion were marked upon the course. In marking these points no separate account was taken of very small twists, indicating a change of direction of less than 45°, unless the length of path was over 2 cm. Any error involved in this omission is hardly greater than that involved in tracing free-hand the course of a subject often in rapid motion. Measurements were made of the length of path and also of the total number of degrees turned through between successive points of inflexion.

The symbol "D" is used in this paper to designate throughout any one series that side of the planarian which is away from the light during directive illumination; the symbol "L" is used to designate the side toward the light during directive illumination.

Table I gives the results of single trials upon eight individuals. The horizontal lines of figures for each individual are (1) the initial performance under normal, diffuse, vertical illumination, (2) the performance under intense illumination from the right or left side (as indicated in the column for position of light), and (3) the final performance under normal illumination. The figures in the four columns under "Course Traversed" give the distance in centimeters passed through while turning to the D-side or to the L-side[6] and the total number of degrees turned through in each direction. The average rate of movement is obtained by dividing the total distance by the total time. The double column at the right of the table gives percentages of excess of distance toward the D- or the L-side. The figures represent the excess of distance passed through while turning toward one side over that passed through while turning toward the other side. The percentage values were found by subtracting the smaller distance from the greater and dividing by the total distance. At the bottom of the table are given the average values for the eight individuals together with the mean variations.

Table II gives the same items for the longer series on individuals B and E, where short periods of normal illumination from above were alternated with long periods of directive illumination. It should be observed that the directive illumination after the first period with E is reversed, thus reversing the significance of the terms D and L as referred to left and right. In the last trials with these two animals it was necessary to prod them after they had remained at rest for 2 minutes. In computing the rates of movement, the time of resting before prodding was included, so that the rates in these cases represent more precisely the activity of the animal than the actual movement.

In Table III the periods under directive illumination of the eight individuals are fractionated: The courses were divided into lengths bearing the same proportion to the total length as ten minutes bore to the total time; *i.e.*, into such lengths as would have been traversed in ten minutes had the planarian moved at a constant rate throughout the period. The excess of movement toward either the D- or the L-side is given in percentages of both linear and angular movement; *i.e.*, there are given both the excess of distance passed through while turning toward one side and the total angular distance turned through.

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[6] It should be observed that this is not the same thing as turning from or toward the light, as the animal may be said to turn toward the D-side or the L-side under non-directive illumination. The terms D and L are fixed for any one series as equivalent to left and right, according as the directive light, when applied, is always applied from the left or from the right.

Table IV shows the excess percentages of both linear and angular distances for the entire series on individual B. The periods of non-directive illumination are given as before (Table II); the periods of directive illumination are fractionated.

ADAPTATION TO LIGHT

In this connection it is desired to show that the planarian, after a continued period of turning under directive light, exhibits a marked tendency to turn in the opposite direction under non-directive light.

As a measure of turning there have been taken the excess percentages of distance passed through while turning, as given in Table I.

It might appear at first thought that the proper measure of the predominance of movement toward one side or the other would be the number of degrees turned through. It must be remembered, however, that the planarian under a strong directive light sometimes exhibits a tendency to turn abruptly toward the light. A similar, although less marked, reversal of direction occurs frequently in the period immediately following that under directive illumination. In this manner the planarian may turn rapidly around two or three times, thus passing in a few seconds through as large an angle toward the light as it has turned through away from the light in several minutes of steady gliding. Since it is most probable that these abrupt turns are not the result of the ordinary directive influences under which the planarian turns, but a specialized response to a continued intensive stimulation, it seems fairer to measure the continuous tendency of the animal to rotate, not by the number of degrees of rotation, but by the distance passed through while turning. The reliability of this substitution of linear for angular measure is attested in a few courses where the abrupt turns were almost entirely absent and the percentages obtained in both ways were very nearly equal.[7]

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[7] For example, with individual F the average curvature toward the light was only 1.07 that away from the light; the excess per cent. based on linear measure was 66%; on angular measure, 65%.

All percentages were computed upon both bases, and both, in general, gave the same result, although those from linear measure were the more striking. The average values for initial non-directive, directive, and final non-directive performance are, respectively, as follows: For linear measure, 5% L, 59% D, and 67% L; for angular measure, 11% L, 32% D, and 69% L.

TABLE I

Ind.	Position of light	Course Traversed				Time	Rate	Excess of Distance	
		Distance		Angle					
		cm.		deg.		min.	cm. per min.	%	
		D	L	D	L			D	L
A	Above	6.9	25.5	320.	830.	3.2	10.1		57.
	Right	*305.7*	*9.9*	*7655.*	*740.*	*30.*	*10.5*	*94.*	
	Above	3.9	11.6	120.	430.	0.9	17.2		50.
B	Above	12.4	19.0	815.	1455.	5.	6.3		21.
	Right	*116.0*	*45.9*	*3605.*	*3460.*	*26.5*	*6.1*	*44.*	
	Above	. . .	38.7	. . .	2725.	5.	7.5		100.
C	Above	34.2	29.1	1285.	1060.	5.	12.7	8.	
	Right	*273.2*	*48.7*	*9720.*	*1005.*	*30.*	*10.7*	*70.*	
	Above	. . .	58.2	. . .	1850.	5.	11.6		100.
D	Above	13.1	10.3	485.	450.	5.	4.7	12.	
	Right	*115.0*	*25.6*	*2120.*	*1285.*	*30.*	*4.7*	*64.*	
	Above	6.5	10.9	475.	605.	5.	3.3		21.
E	Above	37.7	29.7	895.	920.	5.	13.5	12.	
	Left	*107.7*	*45.6*	*1845.*	*1580.*	*15.*	*10.2*	*41.*	
	Above	5.5	58.7	125.	1555.	5.	12.8		83.
F	Above	16.5	22.8	520.	600.	5.	7.9		16.
	Left	*156.7*	*31.7*	*4320.*	*930.*	*30.*	*6.3*	*66.*	
	Above	3.1	30.5	125.	1095.	5.	6.7		82.
G	Above	30.6	21.3	940.	780.	5.	10.4	20.	
	Left	*213.0*	*73.8*	*5580.*	*4100.*	*30.*	*9.6*	*49.*	
	Above	2.1	45.1	240.	1135.	5.	9.5		91.
H	Above	27.6	25.4	485.	590.	5.	10.6	4.	
	Left	*193.2*	*71.2*	*5320.*	*3435.*	*30.*	*8.8*	*46.*	
	Above	10.1	48.7	325.	1095.	5.	11.8		66.
Mean	Above	22.7 ±10.1	22.9 ±4.5	593. ±266.	824. ±231.	4.8 ±0.4	9.5 ±2.4		5. ±20.
	L	*185.6* *±61.3*	*44.0* *±16.1*	*5021.* *±2048.*	*2067.* *±1199.*	*27.7* *±3.5*	*8.4* *±2.0*	*59.* *±14.*	
	Above	3.9 ±2.6	38.2 ±16.4	176. ±128.	1311. ±550.	4.5 ±0.9	10.0 ±3.3		67. ±12.

It will be observed that five planarians showed at the start some tendency to "rightness" and three to "leftness." On the average, however, they are predisposed only 5% in the L-direction. Under directive light their turning varies from 41%

(E) to 94% (A) away from the light; *i.e.*, they have, on the average, been changed by the change of conditions from 5% in the L-direction to 59% in the D-direction. Under normal illumination, after exposure to directive light, the turning varies from 21% (D) to 100% (B and C), on the average, 67%, in the opposite direction from that preponderant under directive light.

From these results, it seems safe to conclude that the planarian turns from the light because of a difference of intensity of excitation by the light stimulus impressed upon its two sides. As the animal is exposed continuously to the light, the light-recipient organs on the side toward the light source become more nearly exhausted than those on the other side, *i.e.*, the planarian becomes on this side light-adapted. If now the subject is placed under normal, non-directive illumination, the uniformly intense light will excite the light-adapted side less than the other side, thus producing within the organism the conditions that prevail when the opposite side is more intensely stimulated. Hence the animal turns toward the light-adapted side.

That the turning reactions after and during directive illumination are of the same order is also indicated by the fact that the curvature toward the more excited side is in both cases greater, suggesting that the animal reacts in a positive manner toward the side excited by a relatively (although not absolutely) strong and continued stimulus, just as it responds by reacting toward other intensive, continued stimuli. The difference of curvature,—the ratio equals 1.22,—is, however, perhaps not great enough to be conclusive on this point.

Table II presents the complete series of individuals B and E. These results are interesting as a supplement to the material of Table I, although they also raise a question that can be answered only by further experimentation.

With B, it will be observed that strong illumination on the right side was alternated with normal trials for more than five hours. It will be noticed that the "leftness" under right stimulation and the "rightness" under normal stimulation with right-adaptation increases, in general, throughout the first half of the series and decreases throughout the second half, although the change is not perfectly regular. This fact may mean that at first, while the right-adaptation is in process, the reaction be-

TABLE II

Ind.	Position of Light	Course Traversed				Time	Rate	Excess of Distance	
		Distance		Angle					
		cm.		deg.		min.	cm. per min.	%	
		D	L	D	L			D	L
B	Above	12.4	19.0	815.	1455.	5.	6.3		21.
	Right	*116.0*	*45.9*	*3605.*	*3460.*	*26.5*	*6.1*	*44.*	
	Above	...	37.8	...	2725.	5.	7.5		100.
	Right	*90.9*	*19.5*	*1540.*	*1555.*	*21.*	*5.3*	*65.*	
	Above	1.0	33.2	140.	1190.	5.	6.3		94.
	Right	*85.9*	*5.6*	*1770.*	*540.*	*18.*	*5.1*	*88.*	
	Above	0.6	30.5	95.	1560.	5.	6.2		96.
	Right	*137.1*	*1.8*	*2615.*	*255.*	*27.*	*5.1*	*98.*	
	Above	2.0	28.1	70.	560.	5.	6.0		87.
	Right	*78.8*	*23.6*	*2555.*	*2040.*	*23.*	*4.5*	*54.*	
	Above	...	32.8	...	830.	5.	6.5		100.
	Right	*52.1*	*29.6*	*1680.*	*2515.*	*21.*	*3.9*	*28.*	
	Above	...	28.8	...	940.	7.	4.1		100.
	Right	*36.7*	*30.8*	*1565.*	*1585.*	*19.*	*3.5*	*9.*	
	Above	2.0	19.4	210.	1020.	7.	3.1		81.
	Right	*49.5*	*5.4*	*1305.*	*505.*	*18.*	*3.1*	*80.*	
	Above	13.7	13.6	435.	1235.	5.	4.5	0	0
	Right	*20.2*	*13.9*	*1065.*	*965.*	*16.*	*2.1*	*19.*	
	Above	2.2	11.3	170.	1535.	12.5	1.1		67.
	Right	*64.5*	*9.1*	*2070.*	*510.*	*25.*	*2.9*	*75.*	
	Above	6.0	5.3	670.	565.	5.5	2.1	6.	
E	Above	37.7	29.7	895.	920.	5.	13.5	12.	
	Left	*107.7*	*45.6*	*1845.*	*1580.*	*15.*	*10.2*	*41.*	
	Above	5.5	58.7	125.	1555.	5.	12.8		83.
	Right	*171.0*	*9.2*	*4740.*	*330.*	*17.*	*10.6*	*88.*	
	Above	47.0	5.4	1345.	165.	5.	10.5	80.	
	Right	*115.0*	*5.8*	*3255.*	*425.*	*20.*	*6.0*	*91.*	
	Above	31.0	...	1005.	...	5.	6.2	100.	
	Right	*84.8*	*13.8*	*4550.*	*385.*	*15.*	*6.6*	*72.*	
	Above	31.8	...	1000.	...	5.	6.4	100.	

comes proportionately more pronounced; but that later, with the right-adaptation complete, the left side, which during horizontal illumination by no means remains in complete darkness, but is on the contrary quite strongly illuminated, also becomes partially light-adapted, thus decreasing the difference in excitation between the two sides, and hence the pronounced character of the turning.

After planarian E had become adapted to light on the left side, it was exposed to light in three successive trials on the right side. The animal turned from the light consistently, but, instead of reversing direction under normal illumination, it continued to turn decidedly toward the left. It seems as if, at the end of the second trial, the adaptation on the right side could certainly not be less than that on the left, and the animal might be expected to turn to the right or at least to show no preference for one side over the other. Why the leftness persisted is a question that can be decided only by experiments upon a number of individuals.

The distance percentages in Table III show that during the first total period of lateral stimulation the animal tends to move less and less away from the light, the excess movement from the light being, on the average, 78%, 56%, and 38%, respectively, in the three successive fractions. This change is probably due in the main to increasing light-adaptation, although it may be influenced by compensatory movements and decreased sensitivity under fatigue. It seems safe, however, to say that the effect of light-adaptation reaches a maximum within the first ten minutes' exposure. In planarian E, the effect was very marked at the close of a fifteen-minute period.

It cannot be said that complete adaptation on both sides was reached at any time, although the absence of the usual response under non-directive illumination in the eighth and tenth trials of B (0% and 6% D) suggests that this condition may have been approximated in the preceding four hours of experimentation.

The adaptation appears to be true adaptation in that it is not permanent. After twenty-four hours, C, F, G, and H showed practically no preference for one side over another. A, B, D, and E were too fatigued to respond. After forty-eight hours A and E appeared normal and showed practically no preference

TABLE III

Ind.	Position of Light	Fraction	Excess of Movement				Estimated Time
			% Distance		% Angle		
			D	L	D	L	min.
A	Right	1	90.		85.		10.
		2	100.		100.		10.
		3	92.		72.		10.
B	Right	1	61.		39.		10.
		2	22.			25.	10.
		3	48.			10.	6.5
C	Right	1	97.		96.		10.
		2	72.		88.		10.
		3	29.			17.	10.
D	Right	1	63.		43.		10.
		2	77.		30.		10.
		3	50.			48.	10.
E	Left	1	65.		31.		10.
		2		9.		30.	5.
F	Left	1	78.		72.		10.
		2	67.		65.		10.
		3	54.		53.		10.
G	Left	1	90.		84.		10.
		2	59.		56.		10.
		3		3.		19.	10.
H	Left	1	78.		63.		10.
		2	62.			1.	10.
		3		1.		18.	10.
Mean	L	1	78. ±11.		64. ±20.		
		2	56. ±25.		23. ±44.		
		3	38. ±26.		2. ±35.		

for either side. D was injured and had to be discarded. B was still fatigued. After approximately four days B showed a slight preference for the left.

REVERSAL OF REACTION

It has already been noted that the planarian under directive light frequently turns abruptly toward the source of illumination in a manner, which, by reason of its quickness and suddenness, is quite different from the slower continuous turning from the light. Although less markedly, the animal also tends to reverse direction abruptly during its course under normal illumination after directive light. This does not appear in the course under normal illumination before exposure to directive light. Since the preponderance of abrupt turns in one direction increases the average curvature of the path in that direction, the difference can be stated numerically as follows: For the eight subjects under initial, non-directive light the curvature in the L-direction was 1.08 times that in the D-direction,—*i.e.*, very nearly equal. Under directive illumination the L-curvature was 1.95 times the D-curvature. Afterward, under final, normal light the D-curvature was 1.22 times the L-curvature. Thus we see that the curvature is greatest in the direction of least turning.

This curious type of behavior appears similar to that described by Pearl[8] as the response of the animal to repeated strong mechanical stimulation. After repeatedly giving the negative reaction in response to strong mechanical stimulation of the anterior end, it "finally jerks back with a strong longitudinal contraction, and turns the anterior end through a considerable arc, so that it points in . . . an entirely opposite direction to that of the previous reactions." "This reaction," Pearl continues, "appears as if, after the animal had tried in vain to get away from an uncomfortable stimulus by its ordinary reaction, it finally tries a wild jump in the opposite direction. This curious change in the reaction induced by a repetition of strong stimuli . . . indicates the effect of the organism as a whole upon its reflexes."

It is not, however, by any means necessary to assume that the animal, failing to get relief by the usual negative response, alters its mode of behavior, implying, as this does, consciousness other than the simplest. It is quite conceivable that the abrupt reversals of direction for brief periods, the "wild jumps," are forms of a compensatory movement, which acts as a relief, not for the continued stimulation, but for the continued movement away from the stimulus. Who has not, after prolonged, unaccustomed bending of the body forward in the removal of potatoes from the ground or tacks from the carpet, stood erect and bent the body far backward in order to "take the kink out of his back," before resuming his labors. The planarian directs its course by means of longitudinal muscles, which warp the worm along its long axis, and thus steer it to one side. It is quite possible that these muscles, after the continued contraction involved in prolonged movement to one side, become cramped, and that there follows what is probably a natural physiological co-ordination, when the muscles on the other side contract suddenly and strongly, stretching the fatigued muscles. This explanation involves the assumption of a simple physiological co-ordination, but it avoids the implication of a complex consciousness. Nor is it really incompatible with Pearl's suggestion that it shows " the effect of the organism as a whole upon its reflexes."

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[8] *Quar. Jour. Micr. Sci.*, *46*, 1903, p. 580.

Let us carry further the analogy to muscular cramp. As the amateur agriculturist works farther and farther along the row of potatoes, he pauses oftener and oftener to stretch until a certain point is reached; thereafter, provided the stimulus is strong enough to compel a continuation of the work, he pauses less and less often and presently not at all. The muscles "get set;" he now feels as if he could not straighten out. A similar phenomenon, although not strictly analogous, can be seen in continued walking, where the rests for the unaccustomed walker become more and more numerous, until at last, if he continues, the walker " strikes his stride " and does not rest again.[9] Proceeding upon this analogy, we should expect the compensatory movements of the planarian to increase up to a certain point, then decrease, finally dropping out altogether, provided, of course, that the animal remains active for a sufficiently long time.

The increase in compensatory movement appears in Table III. Here we must consider the percentages based upon angular movement, as those based upon linear movement largely exclude the effect of abrupt turns and were for that reason used in consideration of light-adaptation. There is shown, on the average, a reduction of turning in the D-direction from 64% to 2%. This reduction may be considered as due in part to light-adaptation and in part to an increase in compensation. We have already considered that the effect of light-adaptation upon the

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[9] The writer has a detailed record of a fifty-mile walk, taken by two persons, without any systematic practice in the preceding six months. The stops for rest, although not numerous, became more and more frequent throughout the first part of the route, until the last twenty-two miles, during which no stops at all were made. In this last stage the pauses for water or for inquiries were accompanied by a continuation of the walking movements with the legs.

D-turning was to reduce it from 78% to 38%. Very roughly, then, the effect of compensatory movement may be considered to be proportional to the difference between these two sets of figures; *i.e.*, 14% at first, 36% at the end.

The increase in compensation is greater for many individuals than the average would lead one to expect. Six individuals (B, C, D, E, G, and H) in the last fraction actually turned through a greater angle toward the light than away from it. This change of direction is entirely as might be expected, so long as the preponderance of preceding continuous movement (as measured by the percentages of distance) has been away from the light, for it is obviously intrinsic in compensatory movement that it should be in the opposite direction from the greater amount of movement.

An apparent exception in the case of E may be explained as follows. In the first ten minutes, 65% of D-movement may have resulted in sufficient compensation to reduce the angular excess in the same fraction to 31% in the D-direction and to change the angular excess in the succeeding five minutes to 30% in the L-direction. By this time, however, the light sensitivity may have become so reduced as to reduce the continuous movement almost to zero. The actual distance toward the light passed through in the compensatory turns would account for the positive value of 9% in the L-direction. The explanation in the other cases is obvious.

That compensation finally disappears is shown, it is thought, in Table IV. In the column of angular measure it will be observed that the planarian at first changes in each period from the D- to the L-direction under directive light from the right, but that later the tendency is to turn first in the L- and then in the D-direction. This behavior accords perfectly with our hypothesis of the final lapse of compensation, if we assume that in each of the later periods the planarian is just so fatigued for D-turns that it responds at the start with a high degree of compensation, which, under increased fatigue, soon drops out altogether. That it repeats this course in successive periods may be due to the fact that the interval of turning under normal light in the opposite direction partially rests it.

It thus appears that we can account for all the facts observed by a theory of abrupt turns given in compensation to preceding

TABLE IV

Ind.	Position of Light	Period and Fraction	Excess of Movement				Estimated Time
			% Distance		% Angle		
			D	L	D	L	min.
B	Above	1		21.		28.	5.
	Right	1–1	61.		39.		10.
		1–2	22.			25.	10.
		1–3	48.			10.	6.5
	Above	2		100.		100.	5.
	Right	2–1	87.		59.		10.
		2–2	46.			16.	11.
	Above	3		94.		77.	5.
	Right	3–1	97.		64.		10.
		3–2	81.		38.		8.
	Above	4		96.		89.	5.
	Right	4–1	93.		69.		10.
		4–2	100.		100.		10.
		4–3	100.		100.		7.
	Above	5		87.		78.	5.
	Right	5–1	27.			17.	10.
		5–2	74.		35.		13.
	Above	6		100.		100.	5.
	Right	6–1	27.			40.	10.
		6–2	28.		7.		11.
	Above	7		100.		100.	7.
	Right	7–1		31.		8.	10.
		7–2	52.		11.		9.
	Above	8		81.		66.	7.
	Right	8–1	68.		13.		10.
		8–2	96.		68.		8.
	Above	9	0	0		54.	5.
	Right	9–1		30.		23.	10.
		9–2	100.		100.		6.
	Above	10		67.		80.	12.5
	Right	10–1	39.			24.	10.
		10–2	100.		100.		10.
		10–3	100.		100.		5.
	Above	11	6.		8.		5.5

continuous movement in the opposite direction. Pearl seemed to think that the positive response to continued strong stimulation was the result of the stimulation itself rather than of the movement caused by the stimulation. If Pearl's interpretation were correct, it should be possible after long exposure to directive light to cause the planarian to turn always toward the stimulus. Upon a theory of compensation the rotation could be brought only to neutrality; the sign of the reaction could not be reversed. In the subjects studied the sign of the reaction was certainly not reversed; yet there is no positive evidence that it could not have been. The theory of compensation is offered merely because it is simpler and more comprehensible and, at the same time, it is adequate to all the facts so far observed.

FATIGUE

The effect of general fatigue upon the rate of movement can be observed in the series with both B and E (Table II). Here the rate under either normal or intense directive stimulation decreases almost uniformly; but the rate under intense illumination is almost invariably less than that under normal illumination at the same part of the series. This latter fact appears with all the individuals studied, the average normal rate before and after directive illumination being 9.5 and 10.0 cm. per min., respectively; and during directive illumination, 8.4 cm. per min. This difference agrees with that observed by Parker and Burnett,[10] who found that a rate of 6.7 cm. per min. under vertical illumination was reduced to 6.24, when the planarian was started directly toward the light and continued turning away throughout its course.

CONSCIOUSNESS

Of interest to the comparative psychologist is this question: If we impute consciousness to the planarian in connection with the negative reaction, what is this consciousness like? It is apparent that the response of the animal is not to the absolute intensity of a stimulus, but to some resultant of the relative intensity of two stimuli; for, under the general distribution of light with the intense illumination, the animal moves toward a side which, it would seem, is more strongly excited than that from which it moves away under weak, diffuse illumination. It is not necessary to assume a two-process consciousness involving an opposition of the conscious representatives of the excitations upon both sides. The opposition may be entirely non-conscious, so that the reaction of the animal may be regarded as merely the physical resultant of two unequal, opposing forces, a resultant which might be represented in consciousness by a single process, just as the resultant of red and green stimulation of the retina is accompanied by a single sensation of the quality aroused by the predominant component.

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[10] *Loc. cit.*, p. 384. *Cf.* also Walter, H. E., *loc. cit.*, pp. 56ff., 75ff.

If we push this analogy further, we are led to assume a qualitative difference between the process accompanying the response toward the left and that accompanying the response toward the right,—just as there exists a qualitative difference between red and green. Such a consciousness of "leftness" and "rightness," involving, as it does, an elementary two-quality form of spatial consciousness, it is, nevertheless, not necessary to assume. To be sure, the substitution by the planarian under continued strong stimulation of a reaction toward one side for that toward the other appears to constitute a modification of behavior that comes at least as near implying consciousness as does any other form of behavior in the planarian;[11] and, if we assume that the change in response is the direct result of continued stimulation, we may expect it to be paralleled by a change in conscious content, say, from a simple "get-away-to-the-right" process to a simple "get-away-to-the-left" process. If, however, we accept the notion that the change of response is due merely to muscular compensation, it is no longer necessary to postulate two avoiding reactions aroused by the same stimulus nor the resulting spatial differentiation. If there is a conscious concomitant to the negative reaction, it may be no more than a single "get-away" process.

We may deny the spatial character of the change of content accompanying the reversal of reaction, and yet not deny the change itself. On our simplest hypothesis, we assume a muscular contraction as the result of great fatigue in other muscles,—a co-ordination as complex as that involved in the ordinary turning away from a stimulus and presumably of equal benefit to the organism. If we postulate a "get-away" process for the first reaction, we may assume a "stretch" process, different in quality, for the second. Consciousness would then consist of the mere alternation between two qualitatively different processes, paralleling the alternation between the two kinds of reaction, or possibly merely the flashing in of the processes at the moment of change.[12]

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[11] In unpublished work by Professor Bentley and the writer the "very characteristic" gripping reaction, forming "an integral part of the normal food reaction," described by Pearl, *loc. cit.*, pp. 624ff., has not been observed.

SUMMARY

Planarians, if subjected to continued, intensive, directive light, continue turning from the source of light. If they are immediately placed in non-directive light after being in directive light, they turn consistently in the direction from which the light at first came. This behavior indicates that the planarian becomes light-adapted on one side during the exposure to directive light, and later, under a uniformly applied stimulus, turns toward the light-adapted side, because that side, on account of its decreased sensitivity, is excited to a less degree than the other, just as would be the case were the animal subjected to directive light from the other side.

After continued turning in one direction under directive light, the planarian tends to make frequent abrupt changes of direction toward the light. These abrupt turns increase in frequency for a while, but, if the stimulation is very long continued, may finally disappear altogether. They may be explained as a form of compensatory muscular movement, initiated as a relief from the continued contraction of the muscles already involved in turning.

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[12] An attempt was made to duplicate planarian conditions upon two human observers, who, with only diffuse light admitted to the eyes through a tracing-cloth blind, the left eye in addition very darkly screened, an electric light fastened to the body in front of the right shoulder, and the arms and legs bound so as to admit only of very limited movement, were instructed "to leave the mind as blank as possible, to move in response to any strong impulse that might be felt, and, when not strongly impelled to do otherwise, to turn from the light." The result was a slow shuffling forward to the left with a tendency, when interrupted by an object, to move backward (compensatory movement?) for a little before going forward again. The introspections showed a scant consciousness, with the left-forward movement carried throughout in unclear kinesthetic terms. At the pauses, there was a heightened consciousness, with an alternation between the left-forward and backward impulses, both carried kinesthetically, in which the latter often supplanted the former, with accompanying backward movement. Although the kind of muscular movement involved is not exactly analogous to that in the planarian, it is believed that the similarity of the introspective account to that derived for the planarian argues in favor of the latter. *Cf.* a preliminary report on *The Use of the Maze in Comparative Psychology*, by L. M. Day and the writer, *Psych. Bull.*, *9*, 1912, p. 60.

The rate of movement decreases with the increase of general fatigue, and is less under intense, opposing, directive illumination than under weak, non-directive illumination.

The conscious experience, concomitant with the turning from the light and the sudden reversals toward the light,—if there is consciousness involved at all,—is not necessarily of a more complex form than that of an alternation between two qualitatively different processes, or a flashing in and out of these processes at the moment of the change of reaction.

THE MAMMALIAN EYE

S. B. VINCENT

From the Psychological Laboratory of the University of Chicago

For the past ten years comparative psychology has busied itself with animal experimentation and in such experimentation, directly or indirectly, vision has played a leading rôle. Whether the problem has been one of discrimination, imitation, or the acquisition of some co-ordination like the maze, sight has been always a factor, if not the important factor with which to reckon. In spite of this, knowledge, structural or functional, of the sense involved has been far from adequate if we may judge from the way the work has been conducted and from the results which have been expected.

It has been generally agreed that the vision of the white rat is poor, yet in looking for anatomical, physiological or psychological data to support such contention or to show in what respects it was poor, where it failed, no entirely satisfactory answer was found anywhere.

On the side of psychology, Mr. Small, speaking of some feats of climbing before the eyes of the rats were open says, "Now as the same movements are used in orientation after the eyes are open as before one is led to conclude that the influence of the eye in the orienting process is very slight." [1]

Mr. Watson thinks vision plays little part in the maze association. He says, "Many times we have taken our normal rats and have flashed strong lights in their eyes. We never got the slightest quiver of an eyelash by so doing much less reaction of the animal as a whole." [2]

Mr. Yerkes concludes that sight is of little importance in the life of the dancer, "that movements and changes in brightness are the only visual conditions which to any considerable extent control the activity of the animal."[3]

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[1] Small, W. S. Development of the White Rat. *Amer. Jour. of Psych.*, 1899, Vol. 11, p. 63.

[2] Watson, J. B. Kinaesthetic and organic sensations: their rôle in the reactions of the white rat to the maze. *Psychological Review, Mon. Sup.*, 1907, Vol. 8, No. 2, p. 90.

Mr Waugh[4] has studied vision in the white mouse. Neglecting for the time his experiments and conclusions on color vision let us consider those on form and distance. The forms to be discriminated were a circle and a cross, cut in black cardboard and illuminated by lights from behind. He only finds discrimination present in one instance, and in this case the per cent. of right choices in 130 trials was only 69 +. He does not give us the exact order of alternating the forms. May not this rat have partly learned the order?

He tested distance discrimination by having animals jump from a wooden disk set at different heights above a table whose surface was varied in color. The time elapsing between the placing of the animal and the jump was taken as an evidence of the animal's ability to discriminate distance. In the first experiment the time increased with the distance, but in the second and third, where there was a glass surface over the table below, the time was unusually long even when the distance was not increased above 10 cm.

Mr. Waugh anticipates the objections to taking the time of the reaction as proof of the discrimination of distance, but he says nothing about olfaction which is also a distance sense and probably a very keen one in these animals. The very fact that glass, which is as non-odorous as anything we know, should present such difficulties would suggest an odor criterion in this case.

He speaks of the short range of vision in the mouse, but gives no anatomical data to support this belief and his experiments with depth give none.

His results were practically negative so far as the discrimination of either form, distance or depth were concerned, but he neglected, as others who have experimented with animals have neglected, to establish first a normal focal distance or distance of accommodation. The distances at which Mr. Waugh tried his animals for discrimination of depth were 2, 4, and 6 in.

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[3] Yerkes, R. M. The Dancing Mouse. New York, 1907, p. 198.

[4] Waugh, K. T. The Rôle of Vision in the Mental Life of the Mouse. *Jour. of Comp. Neur. and Psych.*, 1910, Vol. 20, p. 570.

Hypermetropic animals, and probably all animals are hypermetropic in the wild state, may well fail to discriminate form at short range. The same criticism would apply in a lesser way to discriminations of brightness and color, especially where the differences are slight.

Mr. Watson's rats [5] and Miss Allen's guinea pigs [6] failed to discriminate food at sight and experiments with the dancing mouse by Mr. Yerkes [7] for discrimination of form gave negative results, but Messers. Thorndike [8] and Kinnaman [9] obtained discrimination of form from monkeys and Porter [10] and Rouse [11] from sparrows, cow-birds and pigeons. The monkey's eye anatomically is very similar to that of man and birds possess a well defined fovea, sensitive area and a retina with both rods and cones. A fovea is most likely essential to form discrimination although in discussing the lack of such power, as has been said, we may have to face the charge of providing forms for discrimination which are entirely strange to the animal's life and habitual discriminations if it makes any.

These studies seemed to confirm the general belief that the vision of these animals was not of the strongest, still we were not shown where the weakness lay. The following tabulation is the result of a search for such anatomical, physiological material and gives in this form, so far as found, the answer sought. The facts are compiled chiefly from the work of Chievitz,[12] Lindsay Johnson,[13] Slonaker,[14] and Harris.[15]

Dr. Johnson not only examined the eyes of the animals with the ophthalmoscope but determined the refraction by retinoscopy and measured the angle of divergence of the optic axes with a specially devised goniometer. Chievitz determined the sensitive area microscopically for many animals, including 20 mammals. Johnson with the ophthalmoscope for 182 mammals and Slonaker for 93 vertebrates including 18 mammals. Harris bases his discussion of binocular and stereoscopic vision upon evidence of complete or incomplete decussation of the optic nerves. He studied the ocular movements and the pupil light reflexes and gathered his evidence from operative experimentation and from examination of normal animals.

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[5] Watson, J. B. Animal Education 1903, p. 58.

[6] Allen, Jessie. Association in the Guinea Pig. *Jour. of Comp. Neur. and Psych.*, 1904, Vol. 14.

[7] Yerkes, R. M. *Op. cit.*, p. 180.

[8] Thorndike, E. L. Imitation in Monkeys. *Amer. Jour. of Psych.*, 1902, Vol. 13, p. 98. Animal Intelligence, New York, 1911.

[9] Kinnaman, A. J. Mental Life of two Macacus Monkeys in Captivity. *Amer. Jour. Psych.*, 1902, Vol. 13, p. 98.

[10] Porter, J. P. The English Sparrow and Other Birds. *Amer. Jour. Psych.*, 1906, Vol. 17, p. 264.

[11] Rouse, J. E. The Mental Life of the Domestic Pigeon. *Harvard Psychological Studies*, 1906, Vol. 11, p. 581.

[12] Chievitz, J. H. Ueber das Vorkommen der Area Centralis Retinae. *Arch. f. Anat. u. Physiol.*, Anat. Abtlg., 1891, p. 311.

[13] Johnson, G. L. Contributions to the Comparative Anatomy of the Mammalian Eye. *Phil. Trans. Roy. Soc.*, 1901, Vol. 194 (B), pp. 1–82.

[14] Slonaker, J. P. A Comparative Study of the Area of Acute Vision. *Jour. of Morph.*, 1897, Vol. 13, pp. 445–494.

[15] Harris, W. H. Binocular and Stereoscopic Vision. *Brain*, 1904, Vol. 27, p. 106.

The tabulation includes facts from other mammals for purposes of comparison, and those animals have been chosen which have been most used in comparative psychology. A discussion of the data may be useful since there are so many different factors which help to make vision good or bad.

The color of the fundus is the first thing noticed in an ophthalmoscopic inspection. As will be seen it varies greatly in different orders and species. The significance of this color is as yet but imperfectly understood, although it is thought that it may prove to be in some way connected with the color vision of the animal.

Another thing which the ophthalmoscope reveals at once is the shape and size of the optic disc. Large discs and opaque nerve fibres mean blind areas on the portion of the visual field affected and hence must be noted among the facts which contribute to poor vision.

None of the mammals below the primates possess foveas, but there is often a definite sensitive area whose size and position is important for vision. This has been determined by Johnson for a number of forms with the ophthalmoscope and microscopically for many forms by Chievitz, Slonaker and others. It seems to be larger in those animals where the divergence of the axes is great and when combined with nearly spherical lenses and large corneas it gives the animal a wide range of vision which may possibly be binocular. Probably considering the mode of life such wide range is of more importance than the sharp focus made possible by a fovea; but the fovea, when it does appear, is accompanied by only slight divergence of the optic axes—a condition which most nearly approaches the human for sharp vision.

Acute sight, however, depends also upon the shape of the lens, cornea and pupil. A large cornea permits more oblique rays to enter the eye, while large spherical lenses correct the distortion which might result from the unequal refraction. The size of the pupil is perhaps of not so much importance as its power of dilation. The round pupil is characteristic of the higher types of eyes. It has been found that horizontally oval or round forms react less readily to mydriatics and less quickly to light than vertically oval or round forms. The pupils of ungulates are very unresponsive to either drugs or light.

We need not pause long to consider divergence. Laterally placed eyes, of course, preclude stereoscopic vision and parallel axes are found only in man and monkeys. The divergence is greatest, according to Johnson, in rabbits where the fields of the two eyes probably overlap both in front and behind. It is said that a squirrel can see any one approaching directly behind it.

This ability to see on all sides gives the animal greater security from enemies and is accompanied in the wild state by hypermetropia in all animals below simia, according to Johnson. But both he and others say that refraction becomes variable under domestication.

Johnson also remarks that there is complete absence of convergence below simia and that monkeys converge for only a slight period. He believes that animals below simia do not move their eyes for visual purposes or for purposes of convergence. Harris disputes this and contends that there is a degree of convergence and of binocular vision especially in animals which pursue their prey and have to follow it with their eyes.

Harris studied stereoscopic and binocular vision by means of experimental work to demonstrate the incomplete or complete decussation of the optic nerve fibers and also made microscopic studies of degeneration. He believes that where there is incomplete decussation there is at least the possibility of binocular vision. In these cases the eyes are set rather far forward. He also used the light reflex to confirm nerve section and says that "Sluggishness of pupil reaction seems generally to be accompanied by a low standard of acuteness of vision as in ungulates and rodents."

Popular belief as to the significance of the rods and cones seems to ascribe to the one the specific function of vision in a dim light, colorless vision, and to the other in some hypothetical way color vision. The facts of peripheral vision, color blindness and night vision are brought to the support of this theory. More recent work seems to be greatly modifying the old conceptions of retinal structure and making the distinctions between rods and cones less absolute. The reader is referred to the studies of Hess,[16] Cajal,[17] Bernard,[18] and Cameron.[19]

When the figures giving the refractive indices for lens and eye media are noted it will be seen that there can be little power of accommodation in most of these forms where the refractive indices so nearly approach each other. The figures for man are lens (total refractive index) 1.437, fluid media, 1.336. In fishes the accommodation is brought about by altering the distance between the lens and retina. The rat has no known mechanism for any such purpose nor has any such alteration been observed.

In the light of this discussion what shall we conclude about the rat's eye. As arguing against its good vision we have a thread-like optic nerve, opaque nerve fibres, no sensitive area, no fovea, no eye movements demonstrable,[20] a pupil which reacts very sluggishly to light, a divergence of the optical axes of 60 to 84 degrees, and in the wild state extreme hypermetropia. That there may be an occasional variant from this last under conditions of domestication is true, but the great curvature of the cornea and sphericity of the lens are indications against its being of frequent occurrence.

And what shall we say as to the conduct of experimentation? First, we must establish for all experimentation employing vis-

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[16] Hess, R. Ueber der Bau und Zapfen der Wirbeltiere. *Deutsch zool. Ges.*, 1903, Bd. 13, S. 33–41. See also numerous papers of C. Hess.

[17] Cajal, Ramôn y. Histologie du Système Nerveaux de l'Homme et des Vertébres, Fr. ed., 1911.

[18] Bernard, Henry M. Studies in the Retina. *Quarterly Jour. Mikro. Sc. N. S.*, 1903, Vol. 46, pp. 25–75; Vol. 47, p. 303.

[19] Cameron, John. Farther Researches on the Rods and Cones of the Retina. *Jour. Anat. and Physiol.*, 1911, Vol. 46, pp. 45–53.

[20] I have never seen a rat's eye move under normal conditions. A very slight nystagmus may be produced by rotation showing that there is some power of movement.

TABULATED DESCRIPTION OF THE FACTS OF VISION IN THE EYES OF A FEW MAMMALS

	FUNDUS AND TAPETUM-LUCIDUM	SHAPE AND COLOR OF OPTIC DISC	SENSITIVE AREA	FOVEA	SHAPE OF PUPIL	DIVERGENCE OF OPTIC AXES	REFRACTION	STEREOSCOPIC VISION	RODS AND CONES	DECUSSATION OF OPTIC NERVE	PUPIL LIGHT REFLEX	REFRACTIVE INDEX OF LENS [21]	REFRACTIVE INDEX OF FLUID MEDIA
Primates, (Macacus cereopithicus)	Dark chocolate bounded by darker macular area surrounded by bright circular reflex ring.	Round or vertically oval pink disc, semi-opaque nerve fibers.	Small compared with other mammals.	4 mm toward nasal side of optic nerve.	Circular.	Parallel.	Myopia exceedingly rare, practically no astigmatism.	Simians the only mammals which possess the power of convergence. Monkeys only able to converge for a very short period.	Present.		Quick reflex. Consensual reaction.	1.3338 variation .0031.	1.33322 variation .0032
Carnivora, (Felis domestica)	Several zones, center usually golden yellow, intermediate green, periphery red, brown or purple, stippled with minute dots.	Cup disc, round, varying in color, always surrounded by bright colored fringe, usually emerald green.	Round, toward temporal side of nerve entrance.	Fovea (Krause)	Round, contracting to vertically oval or straight line	7 to 9 degrees.	No myopia in wild state, great variability under domestication, astigmatism.	Complete absence of power of convergence among wild animals, and domestic animals, unless specially trained, do not move their eyes either for convergence or for visual purposes—no habitual convergence below Simiae.	Present, rods and cones palisadal, of same height	Incomplete decussation.	Quick light reflex. Fair consensual reaction, better than dog	1.33006 variation .00407	1.33009 variation .00406
Carnivora, (Canis familiaris)	Same as above; characteristic of night animals; great variation in domestic dogs.	Great variation.	Round area toward temporal side of nerve entrance.	None.	Round.	15 to 25 degrees.	Same as above.	See above.	Present.		Sluggish pupil reflex. Fair consensual reaction.	1.33483 variation .0003.	1.3349 variation .0005.
Carnivora, (P lotor)	Central area covered with yellow patches, darker periphery.	Vertically oval.	Band-like area, horizontally across retina above nerve entrance; 5 to 7 mm.	None.	Circular, concentric arrangement of lens fibers.	25 to 30 degrees.		See above.	Present.			1.3338 variation .00163.	1.33378 variation .00105.
Ungulata, (P equidae)	Central zone of yellow and green, stippled with purple and brown, peripheral zone violet and brown, variable	Horizontally oval, pink, opaque nerve fibers.		None	Horizontally oval, iris hardly sensitive to mydriatics but sensitive to strong light.	40 degrees.	Myopia most prevalent, astigmatism.	See above	Cones long oval, much shorter than rods	About one-sixth of the fibers do not cross.	Sluggish pupil reflex to light. Contraction much greater on side stimulated.		
Rodentia, (Myorpha)	Fundus uniform in color, no tapetum-lucidum, (albino pure white).	Small, chalky white, in rat reduced to a mere point, opaque nerve fibers, optic nerve hardly the size of a cotton thread.	No area	None.	Round, contracting to a pin's head, lens made up of concentric circles, cornea perfect hemisphere extending to center of globe.	60 to 64 degrees.	Rodents markedly hypermetropic in wild state, great variability in refraction and astigmatism under domestication.	See above	Rodents without cones.	Practically complete decussation.	Sluggish reaction, limited to side stimulated.		
Rodentia, (S ludoviciana)	Pink with tint of gray due to opaque nerve fibers. No tapetum	Elongated, white, depressed, both relatively and absolutely larger than any other animal examined	Small, oval, above and to temporal side of retina.	None.	Lens nearly spherical, pupil invariably round.	60 to 80 degrees	Markedly hypermetropic, 2 to 5 diopters.	See above.	See myorpha.				
Rodentia, (H caviidae)	Buff or slaty gray devoid of retinal vessels. No tapetum	Chalky, white round, large. Opaque nerve fibers		None	Circular.	55 degrees	Variable under domestication	See above	See myorpha.				
Rodentia, (Leporidae)	Vermillion red.	Round, oval white disc, considerably above visual axis. Opaque nerve fibers	Band-like area, horizontally across retina, immediately below nerve entrance.	None.	Circular.	60 to 80 degrees.	Hypermetropic in wild state, marked myopia under domestication, no astigmatism.	See above	See myorpha			1.33045 variation .00361	1.33069 variation .0032.

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[21] Freytag, Gustav. Die Brechungsindices der Linse und der flussigen Augenmedien bei Katze und Beim Kaninchen. *Archiv. f. verg. Opth.*, 1910, Vol. I

ual discrimination normal focal distances for all the animals used if our results are to be at all trustworthy; second, if the conditions so nearly approach those of peripheral vision, the stimulus should preferably be a moving one; third, the light conditions should be those to which the animal is best adapted; and fourth, we should not expect sharp discrimination of form from eyes such as those we have described.

BEHAVIOR OF FIRE-FLIES (PHOTINUS PYRALIS)? WITH SPECIAL REFERENCE TO THE PROBLEM OF ORIENTATION

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INTRODUCTION

The production of light in living tissue, although confined to relatively few species, is widely distributed among living organisms. It is rather common in molds and bacteria and it is found in representatives of all classes of animals including vertebrates where it is especially common among certain groups of fishes.

In some cases, the molds for example, it is exceedingly difficult to conceive of any possible biological significance that luminescence may have. In others it is highly probable that it is closely associated with fundamental phenomena in the life of the organisms. All sorts of suggestions have been offered with reference to the function of the production of light in different species. In some it is considered to be mainly a secondary sexual characteristic; in others it is supposed to serve primarily as a lure for prey; and in still others as protection against enemies. Among the most novel of such suggestions that I have seen is one presented by Dubois, a prominent naturalist, who maintains that certain tropical birds capture fire-flies and fasten them to the soft clay walls of their nests and that the light which they produce while thus situated protects the nest against snakes.

While some of the hypotheses which have been offered will probably eventually be found to hold for many different species, only in very few cases has the function of luminescence thus far been well founded, in fact, it has been clearly established only in one case.[1] Galloway found (1908) that in the annelid,

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[1] After the main part of this paper was completed there appeared an article by McDermott, in the *Canadian Entomologist*, Dec., 1911, in which the author presents strong evidence in support of his contention that luminescence in the fire-flies, Photinus pyralis, P. scintillans, and P. consanguineus, functions primarily as a secondary sexual character. In this interesting paper McDermott also calls attention to observations made by Osten-Sacken on Photinus pyralis in Washington, D. C., 1861. These observations, which were described in the *Stettiner Entomologische Zeitung*, also demonstrate fairly conclusively that in this species the production of light functions in mating and this appears to be the first record bearing on the biological function of this phenomenon that is in any way conclusive.

Odontosyllis enopla, just before mating, which probably occurs periodically about every 26 days during several summer months, the females are found at dusk swimming about near the surface of the water. At first they are only very slightly luminous, but later they quite suddenly become acutely phosphorescent, particularly in the posterior three-fourths of the body. At this phase they swim rapidly through the water in small, luminous circles two inches or more in diameter. "The male first appears as a delicate glint of light, possibly as much as 10 or 15 feet from the luminous female. They do not swim at the surface, as do the females, but come obliquely up from the deeper water. They dart directly for the center of the luminous circle and . . . seize the female with remarkable precision, when she is in the acute stage of phosphorescence." No more light is produced by either individual after mating. Galloway leans toward the opinion that the luminosity in the male is useless and that phosphorescence is a by product of a form of metabolism. I assume he would maintain that the origin of this phenomenon was accidental and had no connection with its present function in mating.

In all other instances the conclusions as to the function of luminescence are either purely speculative or are based mainly upon structural evidence such as the more highly developed and larger eyes in the active male fire-flies as compared with the relatively small ones in the comparatively inactive females. Mangold says (1910, p. 328), "Leider sind wir in der Frage nach der ökologischen Bedeutung des Leuchtens fast ausschliesslich auf Hypothesen angewiesen."

It is not my purpose to review the very extensive literature on the subject of luminescence in organisms in its different phases. This has recently been very well done by Mangold (1910) who has brought together in his bibliography 649 titles, an enormous number, testifying to the general interest in the subject.

MATERIAL

I took up the study of the behavior of fire-flies [2] in the neighborhood of Smithsburg, Western Maryland, early in June, 1911.

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[2] Dr. F. H. Chittenden of the Bureau of Entomology, Washington, D. C., kindly identified a number of specimens of fire-flies captured at Smithsburg and pronounced them to be Photinus pyralis. Among these specimens there were some which were much smaller than others of the same sex, and I was of the opinion that they represented a different species. Dr. Chittenden, however, concluded

At that time the insects were very abundant. Dozens of them could be seen at a time flying about over the lawn and garden every evening. I do not know how early in the spring flight begins but after the first of August there were onlv very few seen, usually not more than two or three each evening. In October some were still seen. They probably continue to appear until prevented by frost.

The two sexes in the species studied are nearly alike. There is ordinarily but little difference in size, and the wings of the female, contrary to what is found in many species, are apparently as fully developed as are those of the male. The eyes, however, are much larger in the male than in the female and the luminous area is also much larger. In the male it covers the entire ventral surface of the three posterior segments, in the female only a portion of the ventral surface of the third. Thus, while the two sexes are similar in general appearances, they can readily be distinguished from each other.

HABITS

During the day time these fire-flies are found in dark crevices or under ground. In the evening when it is still light enough to read readily they come out. The males soon take wing and fly about, glowing fairly regularly at intervals of about five seconds. Owing to lack of proper instruments I was unable to ascertain the intervals very accurately. However, I found none less than four or more than six seconds. The females climb to the tips of blades of grass or onto other objects and usually remain quiet; they do not fly. Among all of the specimens captured on the wing during the entire season not a single female was found. My notes contain a record of sixty-one specimens taken

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that the small ones were dwarfs and that all are of the same species and identical with the fire-flies most common in Washington. If this identification is correct my studies were made on the same species as were those of Osten-Sacken and the major portion of those of McDermott. The reactions of the specimens found at Smithsburg, however, differ in at least two important respects from those of the specimens found at Washington judging from McDermott's description. He maintains (1) that the glow of the female is clearly of longer duration than that of the male; and (2) that there is an interval of nearly five seconds between the glow of the male and that of the female in response to it. I was unable to distinguish any difference in the duration of the glow in the two sexes at Smithsburg, and the interval between the glows is very little if any over one second. It seems quite remarkable that individuals of the same species should differ so much in their response, especially when there is so little difference in their environment as is found between Washington and Western Maryland.

thus, and there were probably fully as many more captured on the wing of which no record was kept. The wings of the females are, however, as previously stated, fully developed and they appear to have no difficulty in flying.

The males usually start to glow about ten minutes earlier in the evening than the females. The average time of the first glow for seven different days, not successive, between July 12th and 30th was 7:40½ P. M. for the males and 7:51 P. M. for the females. The time of the day the first glow appeared was surprisingly constant throughout the season. It was recorded on eighteen days between July 4th and 31st and on nine of these days it appeared between 7:35 and 7:40 P. M. Throughout the entire time the variation was only from 7:35 to 8 P. M.

I have no evidence indicating that the fire-flies, as might be expected appear earlier on dark, cloudy and rainy days than on bright, clear days. Low temperature, however, seems to retard their appearance. Specimens kept in darkness or in low light intensity during the day did not become active earlier than those under normal conditions. Males in darkness were never seen to glow spontaneously during the day, although the females were found to respond to artificial flashes of light at all times, and on one occasion after several responses of females to artificial light, a male nearby became slightly luminous and then ran about in an apparently excited manner and glowed a few times.

From the time the first flash of light for the evening is seen in the open the number of glows gradually increases until a maximum is reached between 8:30 and 9 P. M., after which there is a rather rapid decrease. Comparatively few flashes were seen after 9:30 P. M. and only rarely one after 11, although occasionally one was seen after midnight.

Thus it appears that the activity of these creatures is a periodic phenomenon to a considerable extent independent of immediate environmental changes.

REACTIONS

The male insects fly about in a leisurely sort of way, generally from one to two meters above the ground and glow as previously stated, at fairly regular intervals of approximately five seconds. The relatively long abdomen hangs down nearly vertically, with the posterior end turned slightly up, so as to fully expose the luminous tissue to creatures below. The females, on the other hand, ordinarily remain quiet and do not glow unless they are stimulated. If a male glows within a radius of five or six meters of a female she usually responds almost immediately[3] by producing a flash of light. The male then turns directly toward her in his course and soon glows again. Following this the female again responds by glowing and the male again apparently takes his bearing, turns and directs his course toward her. This is repeated usually not more than two or three times before he gets near her and lights. Then he runs about in an apparently excited manner, glowing at irregular intervals, but orienting after each response of the female and proceeding toward her until his antennae come in contact with her, after which copulation takes place at once and all luminescence ceases. It is a very simple matter to follow a series of responses like this ending in copulation. The whole process as described was seen in very many different cases. There cannot be the slightest doubt as to the significance of the production of light in the creature studied.

If there is no wind the male usually lights within a few centimeters of the female, sometimes actually striking her. Thus they may come together in a very short time. But if it is windy the male frequently lights some distance from her and, especially if he becomes entangled in grass and other vegetation, as often happens, it may require many flashes of light, responses and reorientations and repeated flights before he actually finds her. After the male lights he apparently becomes much excited, as stated above, and usually runs about waving the antennae quite actively. The intervals between the flashes of light produced are much more irregular than when he is on the wing. During or immediately after each glow he stops, raises the antennae, takes an attitude of apparent attention and remains perfectly quiet until after the female answers with a flash, then he turns until he faces the direction in which the flash occurred and proceeds. If the female does not respond, as frequently happens, he retains the attitude of attention for approximately a second and then continues on his course without turning.

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[3] McDermott says (1911, p. 400): "The answering of the female does not occur immediately after the flash of the male, but at a period—apparently approximately constant for all females of this species—of about three to four seconds after the flash of the male. This slight delay occurred in every normal case of mating observed with this species, pyralis."

The female glows in response to artificial light as well as to the glow of the male. In fact the response is apparently independent of the character of the light. I was able to induce it by flashes of light produced by means of an electric pocket lamp, parlor matches, tallow candles, kerosene lamps, sun light, light reflected from objects of various kinds, light passed through a plate of ruby glass or through a solution of copper sulfate. It is, however, dependent upon the duration of the illumination. The female will respond by glowing only if the light is turned on, left a certain length of time (a fraction of a second) and then turned off. She will not glow if it is not turned off after the proper exposure. This response is then clearly not due to light in itself but to changes in the intensity of the light. But it is equally clear that the essential factor in the response of the female is not changes of intensity but what such changes of intensity ordinarily represent, that is, the male.

A number of the characteristics in the behavior of the fireflies described above, as well as others are clearly brought out in the following quotation from my note book: On July 8 a female was caught at 8:15 P. M. and placed on the end of a pencil. Here she remained for fifteen minutes, almost perfectly quiet, while I moved the pencil about freely in studying the orienting reactions of several males on a sheet of paper. She was then taken to the porch, where I sat a short distance from the railing holding the pencil in my hand. I watched her carefully until 9:15, that is, for forty-five minutes. During this whole time she retained the same position and glowed only in response to flashes of light produced by males which occasionally chanced to fly near. The farthest distance of a male to which she responded was six meters. After I had been on the porch twenty minutes a male, attracted by the glow of the female, flew in and lit on the railing. Here he crawled about for nearly twenty-five minutes, glowing periodically at irregular intervals. One series of successive intervals recorded in seconds reads as follows: 35, 8, 12, 16, 5. The male turned toward the female every time she glowed in response to his illuminations, which was about two out of three times, but he could not get to her without either flying or crawling down to the floor and then up on my leg. He did neither and consequently did not find her, although he was still active when the observations closed at 9:15. At that time the pencil containing the female was stuck into a crack in the wall at the back of the porch, about six feet from the floor, and left. At 2:20 A. M., the following morning, a male was found copulating with her. At 5:15 the male had left but the female was still on the pencil and had not moved. At 6 she was still there but at 7 she had disappeared.

FUNCTION OF LUMINESCENCE

Our description of the behavior of fire-flies demonstrates conclusively that luminescence functions in bringing opposite sexes together for the purpose of copulation. The following experiments show that this is the only factor involved in the phenomenon, that vision and smell play no part in it.

If a female is placed on the end of a pencil or small twig she remains quiet, as stated above, sometimes for many minutes and thus she can be readily transferred to any point desired. In studying the orienting reactions of the males on a sheet of paper I frequently made use of this method in handling the female. Under such conditions both respond normally and if not disturbed they soon come together and copulate.

In these experiments I repeatedly held the female near the male and found that if she did not glow there was no indication of any response unless his antennae actually came in contact with her. Thus I have often seen a male, in running about, come within a centimeter of a female without the slightest appreciable change in his course and then after continuing several centimeters, sometimes twenty or more, turn about in response to her glow and proceed directly toward her. Sometimes, however, the males were seen to turn toward the female when they came near her; but I found that they turned toward the pencil quite as often when it did not contain a female. Consequently, while the turning of the male in these cases is evidently due to the presence of an object it is clear that he does not recognize the female by vision and that images of objects, if such are actually formed, play a very insignificant part in the process of mating.

In some insects mating appears to be controlled very largely by chemical diffusion from the female. Mayer (1900) found this to be true for certain butterflies. The observations described above show that if diffusion of chemicals functions in the mating of the fire-flies studied, the chemicals must be emitted only in connection with the production of light. But that chemicals which may be thus emitted are insignificant in the process of mating is shown by the fact that if the females are enclosed in air-tight glass jars as was repeatedly done, the male finds them quite as readily as when they are free. (See experiment described below.) It is consequently clear that aside from actual contact the flashes of light produced by the fire-flies are alone functional in directing the male to the female in mating.

However, as previously pointed out for the female, so also for the male the responses preliminary to copulation are not primarily due to the effect of light in itself or to the effect of changes in the intensity of light. The responses are clearly given with reference to something which the flashes of light represent, a sexual phenomenon, preservation of the race, etc. Thus the fire-fly, although probably not conscious, acts with reference to the future. It may be said to project its present into the future, to live in the future as well as in the past and the present.

RECOGNITION OF THE FEMALE GLOW BY THE MALE

In many species the illumination produced by the male differs markedly from that produced by the female. In some the glow of the male is much more intense than that of the female. In others there is a marked difference in the duration of the glow of the two sexes. In still others the illumination has certain peculiar distinguishing characteristics, as, e.g., in Photinus sanguineus in which the male glow consists of two flashes of light separated by a short interval, while in the female it consists of but a single flash. In the Western Maryland species[4] I was unable to distinguish between the glow of the two sexes either in the quality or in the intensity of the light or in the duration or the nature of the glow. At times it did, indeed, appear as though the males produced more intense flashes than the females, but if several of each sex were put into different glass jars, placed side by side and observed from a distance of two or three meters, as was repeatedly done, I was never able, by focusing attention on the character and the amount of light alone to distinguish one from the other with any degree of certainty.

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[4] McDermott (1911, p. 400) referring to Photinus pyralis says: "The flash of the female, while of the same colour as that of the male, is easily recognized after a little practice, being slower—or rather of longer duration—and less intense."

The male fire-flies, however, seldom if ever respond to the glow of other males. In some way they appear to be able to distinguish between the flashes of light produced by the two sexes, as the following experimental observations clearly indicate.

A number of males were put into one glass liter jar and three females into another. Both were sealed and set in the garden 50 cm. apart. Thirty minutes later six free males had collected about the jar containing the females and none around the other jar. The two jars were then interchanged in position. Ten minutes later all of the males had again collected about the jar containing the females. These experiments were repeated with slight modifications on nine different evenings and the results in all cases were essentially like those stated above. Moreover, if a female and several males are put on a plane surface and closely watched, as was repeatedly done, the males seldom if ever make the mistake of turning toward other males. And again that the males recognize the glow of the females is clearly shown by the difference in the effect produced upon them by the glow of the two sexes as shown in the following experiment.

Five males were put into each of two glass liter jars labeled A and B respectively. The jars were then sealed and placed 50 cm. apart on a sheet of paper spread out on the ground. Four females were put into a 50 cc. bottle and placed 50 cm. from each of the two jars. Two free males soon came and lit on the paper. An opaque screen was then so arranged that the glow of the females could be seen by the males in one jar (A) but not by those in the other jar (B). The males in jar (A) and the two free males soon glowed freely, as did also the females. In fact they glowed so freely that there was almost a continuous flashing. The males in jar (B), however, presented a very different spectacle. They were watched carefully for fifteen minutes and during this time produced a total of only three flashes. It should be emphasised here that these males could see the numerous flashes given by the males in the other jar but that they could not see those of the females. The screen was then removed so as to expose the females and it was found that the behavior of the males in jar (B) changed almost immediately. They became much more active and gave every appearance of being much excited; and in three minutes they produced a total of twenty-eight flashes of light as against three in fifteen minutes before the females were exposed. Such a specific stimulation of the male by the glow of the female is very evident to anyone making observations on these creatures and cannot be mistaken. It was observed again and again throughout the season under many different circumstances. A specific effect of the glow of the female is also attested by the fact that I was unable to induce the mating responses in the male by artificial imitation of the flashes of light produced by the females. Many attempts were made mainly with an electric pocket lamp but in only two or three instances was there any response on the part of the males used in the experiments, and in these it was not at all definite, although in practically all cases they responded definitely to the glows of females.

There are several factors that may possibly be involved in this phenomenon of recognition. Let us enumerate them: (1) The female is always at rest when she glows while the male is ordinarily on the wing; (2) the form of the luminous area differs in the two sexes; (3) there is always a certain time relation between the glow of the male and that of the female in response to it; (4) the quality of light produced by the two sexes may differ; (5) the intensity may differ; (6) the duration of the glow may differ.

Many observations and experiments were made in attempting to ascertain which of these factors are of importance in the process in question. I shall first describe the results of some of these and then attempt to interpret them.

(a) The mating responses are quite as precise when both male and female are at rest as they are when the male is on the wing, and the males are no more likely to turn toward other males. It is therefore evident that the fact that the female is at rest while the male is usually in motion when they glow can be of no particular value in the phenomenon of recognition.

(b) At different times, as previously stated, a piece of ruby glass was held between male and female fire-flies so as to change the color of the light flashed from one to the other. This change produced no apparent effect on the mating responses of either. The female answered the male as promptly and the male turned toward the female as precisely as they did under normal conditions. The same was found to be true when the ruby glass plate was replaced by a flat flask filled with copper sulfate, or with a cylindrical bottle filled with water, or with one, two, three, or four thicknesses of paraffin tissue paper used in wrapping baker's goods.

These results seem to show that the recognition of the female by the male may be independent of any possible difference in the quality or in the intensity of the light produced by the two sexes, or of any possible difference in the form of the luminous area. Moreover the fact that males respond to females at various distances indicates that intensity cannot be of any special importance in this, and the fact that males hidden from females were often seen to turn toward glass jars or white paper from which the light produced by the females was reflected indicates the same with reference to form. We have left then as possible distinguishing characteristics seized upon by the male in the process in question, the time relation between the glow of the male and the response of the female and possible difference in the duration of the glow in the two sexes.

(c) Thirteen males were put into one glass liter jar and three females into another. These jars were then sealed and placed 25 cm. apart on a sheet of paper spread out in an open place in the garden. Ten minutes later three free males had collected about the jar containing the females and none around the other jar, although the thirteen males produced many more flashes of light than the three females.

One of the free males was captured and so oriented on the sheet of paper that he proceeded toward point very nearly midway between the two jars. He was rather sluggish and moved along slowly without glowing. When he reached a point nearly between the jars one of the males in the jar glowed. This did not appear to affect him in the least for he continued on his course without any apparent change until a female in the other jar answered the glow of the male, then he suddenly turned almost at once directly toward the female and immediately became more active and apparently considerably excited. Shortly after turning he emitted a flash of light but he did not stop while he glowed as is ordinarily the case when the males are not on the wing. Similar reactions were seen many times in experiments made for other purposes so that there can be no question as to the accuracy of these observations. Moreover this experiment was repeated several times with different individuals and essentially the same responses were obtained in all.

In a few instances a free male turned toward the glow of a male in the jar, but the response was always much slower and less definite than an orienting response to a female, and invariably, if a female glowed after he had started toward the jar containing the males he turned about at once and proceeded toward her, whereas there was never the slightest indication of such a response to a male glow after he had once become directed toward a female.

These observations, especially the fact that the male, without having glowed at all, sometimes turns at once with scarcely any interval, toward the flash of light produced by the female, seems to indicate quite conclusively that the relation in time between the glow of the two sexes, that is, the fact that the glow of the female usually follows that of the male at a given interval, is not of prime importance in the recognition of the female by the male.

Thus we have excluded all characteristic differences between the response of the two sexes which might serve the male in distinguishing between them in the mating reactions save possible difference in the duration of the glow. But as was formerly stated no difference in duration could be definitely established. If the male, then, finds the female owing to unequal duration in the glow of the two sexes, as is probable, he must be so adjusted that he can detect differences too minute to be readily distinguished by the human eye.

ORIENTATION

The luminous or photogenic tissue in the female as previously stated is confined to a small area on the ventral surface of the third segment from the posterior end. Just before the female glows in response to a flash of light produced by a male she raises and twists the abdomen so as to direct the ventral surface, the source of light, toward the male. If the male is directly above she twists the abdomen nearly through 180 degrees, if to the right or left through 90 degrees, etc. Only when the male is directly below does the female glow without first twisting the abdomen. These responses occur, in the Western Maryland species, almost invariably and they are so striking that they cannot readily be overlooked.

In thus directing the luminous surface of the abdomen toward the male no matter where he is located, it is evident that the character of the response must depend upon the location of the stimulus. It is markedly different when the light produced by the male strikes the anterior surface of the eyes than when it strikes the posterior or any other surface. To put it anthropomorphically, the female acts as though she recognized the spacial position of the male. In these responses the female does not begin to twist the abdomen until some little time after the male glows. Her reaction takes place in the total absence of the stimulating agent. After the flash of light which induced the response has vanished she directs the ventral surface of the abdomen toward the place where it was. It is therefore evident that the process of orientation in the female, (a response resulting in a position of the abdomen bearing a specific relation to the location of the male), is not regulated by the continuous action of the stimulating agent as is demanded by Loeb's theories of orientation (tropisms). The reaction is clearly due to change in light intensity; but it is without meaning unless it is referred to what the changes which induce the response represent, that is, the male and all subsequent occurrences in the process of reproduction.

In the male we find responses very much like those just described. The female produces a flash of light in answer to the glow of a male; then after a short interval, he turns, no matter what his axial position may be, until he is directed toward her and then proceeds. Thus if before the reaction he chances to be directed from the female he turns through 180 degrees, if at right angles to her through 90 degrees, etc. If the female is suddenly removed after she glows the response of the male is precisely the same. He turns through the proper angle and proceeds toward the spot where the female was when she glowed.

The accuracy of orientation in these reactions is really surprising. In making observations with reference to this point the female was usually put into a glass vial 2 cm. in diameter. This was set over a circle of equal size on a sheet of paper spread out on the ground. Males flying about were ordinarily soon attracted by the female and lit on the paper, sometimes very near the vial or on it, at other times 30 to 40 cm. away. In orienting after they lit they seldom failed to become directed toward a point within the circle under the vial no matter whether the female was removed immediately after she glowed or whether she was not removed at all.

The male usually does not begin to turn toward the female in response to her glow until the illumination has entirely disappeared. Thus, in the total absence of the flash of light which caused the reaction he turns through the proper angle, that is, he orients and proceeds toward the spot where the flash occurred. How is this possible? Does it mean that the male fire-fly can locate objects in space, that he has a spacial sense, that he knows and remembers the extent of turning necessary in order to direct his longitudinal axis toward any given point in space?

If we assume, just as in the case of the female, that there is in the male a specific response for the illumination of every surface of the eye; that is, that momentary illumination of the posterior surface is followed by turning through 180 degrees and then locomotion straight forward, the side of the eye by turning through 90 degrees and then forward movement, the front of the eye by forward movement alone, etc. It is not difficult to conceive all of these reactions to be purely mechanical, reactions of the nature of unconscious reflexes.

Whether or not the production of light in these insects is a sort of by product of metabolic changes, as maintained by many, it is evident that it is of fundamental biological significance. Indeed it is safe to say that its elimination would result in the destruction of the race and yet the light in itself can be of no special importance. The essence in the response of the male, like that in the response of the female, is not light but what light represents, that is, an individual of the opposite sex.

Males were at different times exposed in continuous illumination produced by means of a pocket electric lamp, a candle or a kerosene lamp. Only in two or three instances was there any indication of orientation and in these exceptional cases the reactions were very indefinite. These results, together with the fact that orientation in the male ordinarily does not begin until after the flash of light which induces it has vanished, demonstrate conclusively that the process of orientation and the direction of locomotion after orientation are not regulated by the continuous action of light, and that these reactions are consequently not in accord with Loeb's theories of tropisms according to which orientation is, as he puts it (1906, p. 135) "a function of the constant intensity." Stimulation which results in orientation in the fire-flies studied is unquestionably due to changes in light intensity much as it is in Stentor, Euglena and many other similar organisms (see Mast, 1911, pp. 80–205) and the insects remain oriented, not because of continuous action of light in accord with Loeb's theories, but because they tend to take a comparatively straight course in the absence of external stimulation. And since the external stimulating agent does not act continuously in directing this animal on its course after orientation, there can be no *a priori* necessity for assuming that it does in other animals, as Loeb (1911, p. 478), Parker (1912, p. 463) and others appear to hold.

The responses to flashes of light in both the male and the female fire-flies are unquestionably adaptive and an explanation for them must be sought along the same lines as an explanation for any other adaptive characteristic in living organisms. It is needless to add that the aim of practically all scientists in explaining natural phenomena, including the psychic, is to trace the order of events back to a mechanical basis. But we may as well face the facts squarely and realize that with reference to reactions it is questionable whether this has as yet been accomplished in even a single case. The assumptions of Loeb and Bohn to the contrary notwithstanding.

SUMMARY

1. The Western Maryland fire-flies are found in dark crevices or under ground during the day. In the evening, when it is still light enough to read, they come out; the females climb to the tips of grass or onto other objects and remain quiet; the

males fly about and glow fairly regularly at intervals of about five seconds. The females do not glow unless light from the males or from some other source is flashed on them.

2. Immediately before a female glows in response to a male she raises and twists the abdomen, until the luminous area on the ventral surface is directed toward him, no matter where he is located, then he ordinarily turns directly toward her and proceeds. These reactions are repeated until the two come together, after which copulation takes place at once. There is no other factor involved in mating. If a female is held near a male he pays no attention to her unless there is actual contact, showing that neither vision nor smell is functional in mating.

3. The males very rarely respond to the glow of other males or to flashes of artificial light. In some way they are able to distinguish between the illumination produced by the two sexes. Although no marked difference, either in quality or quantity, between the male and the female glow, was detected, experimental results indicate that there is probably a slight difference in duration and that this is the factor primarily involved in the phenomenon in question.

4. The males do not orient when exposed to continuous illumination. They respond only to flashes of light and the reaction does not begin until after the light has disappeared. Removal of the female immediately after she glows has no effect on this reaction. Thus orientation may take place in total darkness, and it is surprising how accurately these animals turn through the proper angle in the total absence of the stimulating agent that caused the response. Here we have a case in which it is clearly demonstrated that light does not act continuously in the process of orientation as demanded by Loeb's theories, a case in which it is also clearly demonstrated that continuous action of the stimulating agent is not necessary to keep the organisms oriented.

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OBSERVATIONS ON DOVES LEARNING TO DRINK

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The method of learning to drink has been studied carefully in fowls[1] and in a few other birds whose drinking resembles that of fowls.[2] But the drinking of pigeons differs from that of fowls in two important respects as follows: (1) Fowls and most birds drink by taking a little water in the mouth, then throwing the head up and swallowing this water, then lowering the head to refill the mouth, and so on. In contrast to this, all the pigeons (Columbidae), when they drink, hold the beak submerged in the water and gulp the liquid continuously. When it has finished and withdrawn the bill, the pigeon usually stands still, mumbling the bill a little and swallowing in a manner adapted to drain the throat and mouth; but even this part of the process has little resemblance to the drinking of fowls, for it is a very slight activity and the head is not raised above the normal. (2) The chick is precocial and must learn to drink soon after hatching. The pigeon is altricial, it is supplied by its parents with moistened food. It need not learn to drink for two weeks or more (the time varying somewhat with the species) and even after this period its learning to drink may be slow and gradual.

The following observations were made on the blond ring-dove, *Turtur risorius*.

OBSERVATIONS ON DOVE NO. 43

This dove was hatched on December 17, 1910. It was fed plentifully by the parents for about 24 days, after which the parents were unwilling to feed it. By that time it had learned to eat seed quite proficiently, but had had no opportunity to learn to drink. It was then taken away from the parents and brought up by hand. It ate much seed for itself, but in addition soft food was placed by hand in its mouth and water was given it by means of a medicine dropper. It was not offered water in a dish until the 28th day, from which date its diary reads as follows:

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[1] Breed, Frederick S. The Development of Certain Instincts and Habits in Chicks. *Behavior Monographs*, No. 1, 1911. pp. 8–13 are on drinking.

[2] Morgan, C. Lloyd. Habit and Instinct. London, 1896. pp. 44–46.

28*th day.*—It has had no water since last night, but has eaten a good deal of seed, must be thirsty. I give it water in the dish from which it is accustomed to eat seed. To attract its attention, I take water up in spoon (slowly, so as not to frighten it) and drop the water again and again into dish. Dove watches the dropping water with indifference. I then put many seeds into the water and await results. Dove goes to dish in its accustomed manner and begins to peck at the seeds. Getting the tip of its bill wet, it shows surprise, starts back and shakes head, throwing the water off. Then pecks at seed again, with same result, but the shock of surprise a little less sharp. This repeated a great number of times; also, the dove sat on the edge of dish and then walked in the water; but it does not drink. This evening it was given a little water from medicine dropper.

29*th day.*—I tried the experiment as before, with same results, the dove withdrawing its bill with apparent displeasure whenever it touches the water. Yet it must be thirsty.

30*th day.*—To see if it will drink by imitating the old ones, I place it near the cage of an adult pair who have had no water since yesterday and who will drink out of a cup as soon as it is offered to them. I place the young so that it will see the two adults drinking directly before it, and can drink out of the same cup if it wishes. Result: The young watches them. As they lower their heads and drink, it pays no attention to this operation; but as they raise their heads again, it begs from them strenuously.

Immediately after this test, I test the young by itself. I decide to use a deeper dish than on the 28th and 29th days; the dish in which it wet its bill before was too shallow for the bill to be wholly immersed, though deep enough for an adult to drink out of it easily. The deeper dish of water being put in, with some seeds in the water, the dove soon comes and picks up a floating seed, withdrawing its bill quickly from the water as before. Then it apparently tries to pick a seed from bottom of dish—when bill thus became immersed, the swallowing reaction was immediately set up, evidently in a very mechanical manner, the neck being strongly decurved, the eye half closing, the whole head and neck rigid, while the mouth gulped slowly, the entire reaction giving the impression that the bird had lost consciousness, or felt, if any consciousness, a painful one.

Conclusion: The young pigeon does not learn to drink by imitating drinking as such. It drinks first by an involuntary reflex act of swallowing when the bill becomes accidentally submerged in water.

31*st day*.—This dove has had no water since its one drink yesterday. 12:30 to 1.00 P. M. I keep the dish of water in its cage, and watch. Dove does not recognize the water, though I drop seed after seed into the water. Finally, about 1 P. M., dove having moved away from dish, I move the dish close to it. Suddenly, I know not why, it thrust bill into water, and immediately the swallowing reaction was set up. After swallowing several gulps, it withdrew bill and stood in rigid erect attitude, making swallowing movements in a stiff, mechanical manner quite unlike that of the adult.

32*nd day*.—I offered it water today in same dish as formerly. I tried for about ten minutes to call its attention to this water, by dropping seeds in, by moving the dish in front of the dove, etc.,—but all failed to attract its attention. Then I took a pencil and thrust the point of it repeatedly into the water. The dove immediately noticed this and began to thrust its bill into the water as the pencil had been thrust in. After two or three such thrusts, the next thrust took its bill deep into the water and the swallowing reaction ensued.

33*rd day*.—1 P. M. I put water dish in, "peck" in it with pencil, and drop in seeds and gravel, and thus succeed, after a time, in drawing dove to the dish. It puts down its bill *beyond* the dish and tries to drink there! But soon (from getting toes wet?) puts bill into dish and drinks.

6 P. M. (Room lighted by incandescent electric lamp.) I offer water, again "pecking" into dish with lead pencil. This keeps stimulating dove's attention. After a time it comes, puts bill down just *outside* the dish and tries to drink. Then walks around dish. I keep drawing its attention by dropping seeds into the water. It seems to want to drink, but does not again really try—until it steps full into the water; then it quickly puts bill in and drinks, as if stimulated by wet feet.

34*th day*.—6:30 P. M. (Electric light.) I put in same dish of water. Dove seemed to pay no special attention to mere presence of dish. But when I "pecked" in dish with pencil, it showed keen attention and activity, came out of its nest and to the dish and immediately drank, beginning the drinking movements apparently before it quite reached the water.

35*th day.*—8 P. M. (Electric light.) I put in same dish of water. Bird immediately shows interest, comes out of nest, walks near the water, back and forth, but does not drink. I then begin tapping in water with pencil. This excites the bird. It makes a slight begging movement toward the fingers holding the pencil. Then it makes a slight drinking movement, outside the dish. But though I tap long with pencil, and move dish about, it does not drink. Then I drop seeds into dish. It gradually concentrates its attention, finally runs at a dash into the dish and puts bill down beyond the dish and tries to drink there. Failing there, it immediately puts bill into dish and drinks a long, deep draft. What caused it thus suddenly to find the water? It may have been wet feet; or it may have been sight (reflection of the electric light?).

36*th day.*—2:30 P. M. I put in same dish of water. This time dove needs no tapping with pencil, no dropping of seeds. It soon concentrates attention, goes to dish, stops a moment, walks into dish and tries to drink from beyond it. Failing in that, it immediately puts bill into dish and drinks.

37*th day.*—10 A. M. Same dish put in. Bird recognizes it at once, walks into the dish, puts bill down beyond dish and tries to drink; seems disappointed and tries thus several times. Then walks beyond dish on floor, walks round to original side of dish, stops, and after a little hesitation and apparent "thought," puts bill down toward water, then into water, and drinks.

8:20 P. M. As I poured water into dish, out of sight of bird, it showed indubitably that it recognized the sound, trying to get out of cage toward source of sound, making slight movements which show thirst. On some previous days I have been quite sure that it did not recognize the sound of dropping water. I put dish in cage. Bird, in nest at other end of cage, reaches out vaguely toward dish, then bends down and tries to drink! This it repeats several times. It then comes down from edge of nest, walks toward dish, tries to drink outside dish, steps into dish, tries again without getting bill into water, soon steps out of dish, and then appears as if not knowing what to do. After a minute or two, turning round, it goes to dish and with little hesitation drinks. Its success now seems due partly to accident, partly to abatement of its excitement.

38*th day.*—9 A. M. I put in same dish. Dove is a little frightened, but soon recovers, then goes straight to dish and drinks Apparently it finds the water more easily by daylight and when wide awake than by lamp light and when not so wide awake. But even now, it drinks from extreme edge of water, at one end, as if its aim were at the dish, not the water.

39*th day.*—This forenoon I put water in cage in a dish of different shape from the customary one, and of glass, whereas the former one was of china. The dove came and drank from it almost immediately. However, I judge that this was chiefly because my hand putting the dish in gave it the cue. To test this, in the afternoon, I put in a tin cup full of water, contriving to keep the bird's attention elsewhere while I put it in. Result: The bird examined the tin cup a good deal, just as it would any strange object, but made no attempt to drink from it. Later I replaced the tin cup by a low tin dish. The two were in the cage altogether for an hour or more; they presented the further advantage that one was new and bright, the other tarnished, dark. Yet the bird tried neither of them. Then I took out the tin dish and put in the usual white dish: the bird drank from it after a few seconds' examination.

55*th day.*—After twenty-five days' experience, I believe this bird still does not recognize water as such.

Whenever the dish is put in, the bird at once puts its head down to drink, though it may not be near the dish; but it comes very soon to the dish and drinks readily from it.

OTHER OBSERVATIONS

Dove No. 46, hatched April 17, 1911, was similarly reared by hand. The history of its learning to drink, which was not recorded in detail, was much like that of No. 43.

In case of birds that have water in the cage all the time it is difficult to say when the young takes it first drink. Dove No. 22 was first observed to drink on the 22nd day. Such birds probably learn to drink more quickly than the subjects of the experiment, which saw water for only a short time each day.

A very young bird, even a nestling, will swallow copiously if it is taken in the hand and held with the bill submerged in water.

CONCLUSIONS

(1) The innate drinking activity of pigeons consists of bending the head down low and swallowing.

(2) The drinking reflex probably has no innate afferent inlet [3] except that of the touch of water on the inside of the mouth. The dove does not instinctively give a drinking response to the sight or sound of water, nor to the touch of water on distal parts of the body. The drawing back when the tip of the bill touches the water is especially interesting. The young dove first gets its bill into water, probably, chiefly by pecking; either pecking at objects in the water which attract its attention, or pecking in imitation of old birds.

(3) It is true that the young dove sometimes made a sudden response apparently to a beam of light on the water or to the touch of water on the feet. But these stimuli were not always effective; when they were effective, I judge that it was merely by catching the bird's attention. Pigeons, like jackdaws and bower-birds, are accustomed to peck at glistening objects.

(4) Though doves instinctively imitate pecking, they do not instinctively imitate drinking as such.

(5) The dove which was studied in most detail, dove No. 43, in drinking the second time (31st day) and the third time (32nd day) showed no evidence of remembering the first experience (30th day). But the fourth time (33rd day), and in all instances thereafter, it showed clear evidence of memory.

(6) In the course of experience the dove may associate the drinking reflex with all sorts of stimuli. Nos. 43 and 46 both associated it especially with the dish in which water was given and with the person who brought the dish. Even after much experience the dove may not recognize water as such.

(7) Doves which had imperfectly formed drinking associations frequently anticipated the reward of their efforts by making drinking movements before going to the water. This throws an interesting light on the purposiveness of animal behavior, and I hope to refer to it in another article.

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[3] This term I take from McDougall, William. An Introduction to Social Psychology. Boston, 1909.

(8) Is there any difference between fowls and pigeons in the respective parts played by instinct and by experience in drinking? We have not sufficient data, especially on pigeons, to answer this question. It would be well to investigate also the drinking of those birds of arid regions of which it is said that they never drink from pools of water but only from hanging drops. Wilson[4] says this of our prairie-hen. Such birds appear to be the extreme opposite of pigeons.

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[4] Wilson. Alexander and Bonaparte, C. L. *American Ornithology.* Article on "Pinnated Grouse." In the edition published in Edinburgh in 1831, this statement is in Vol. II, p. 330–331.

REACTIONS OF CHICKS TO OPTICAL STIMULI

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One figure

In the course of a previous study,[1] in which the rapidity of development, permanency, and interrelation of certain habits in barred Plymouth Rock chicks were tested, the problem suggested itself of determining more accurately the nature of some of the optical stimuli in response to which a chick is able to acquire a habit of reaction. In these earlier experiments the animals were tested in their ability to discriminate two simultaneously presented stimuli differing essentially in color, form, or size. Although certain conclusions as to the *what* of discrimination seemed quite safe, it was thought best to conduct a further investigation in this direction with greater refinement of method. The earlier tests seemed to yield positive results in the discrimination of color and size differences, and negative results in the tests on forms.

The problem, then, became that of determining more accurately whether or not the elements of form, size, and color may become the basis of discrimination for the chick. The work was undertaken and completed in the Harvard Psychological Laboratory under the guidance of Professor Robert M. Yerkes, who proposed the investigation and assisted throughout with invaluable suggestions.

APPARATUS

A perspective of the apparatus employed is shown in figure 1. For a complete description, including dimensions of parts, the reader is referred to an article by L. W. Cole in an earlier number of this journal.[2] Following Cole's description, we may consider the apparatus as composed of three boxes: (1) The hover box, O; (2) the illumination box, covered by the

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[1] Breed, F. S. The development of certain instincts and habits in chicks. *Behavior Monographs*, vol. 1, no. 1, 1911, p. 41 ff.

[2] Cole, L. W. The relation of strength of stimulus to rate of learning in the chick. *The Journal of Animal Behavior*, vol. 1, no. 2, 1911, p. 112 ff.

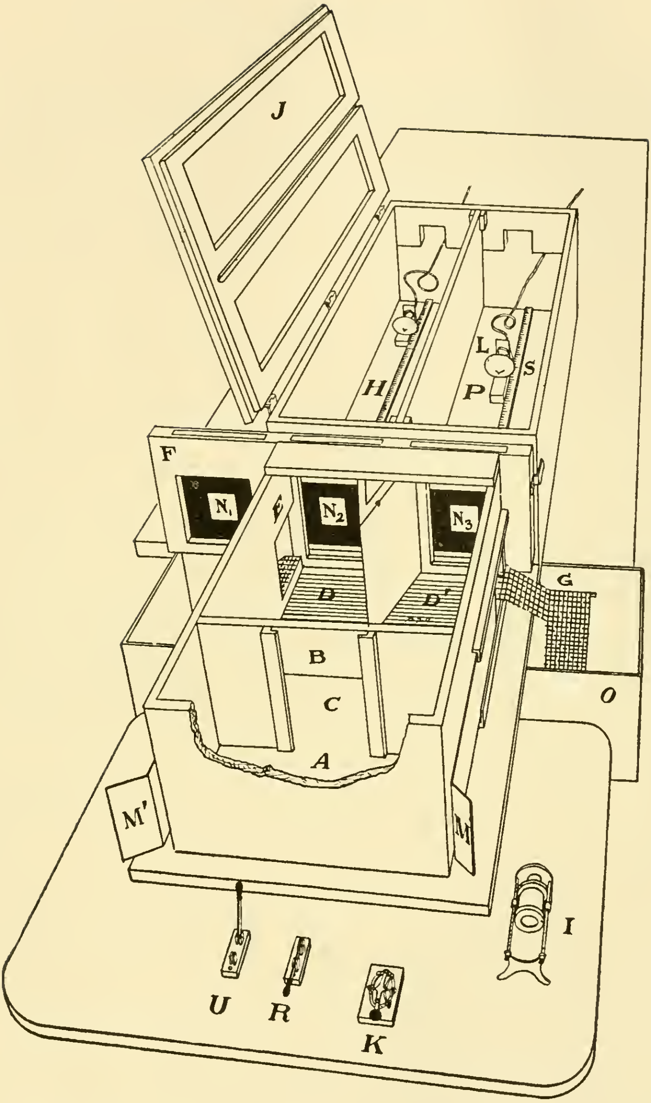

FIGURE 1. Perspective of apparatus used in testing reactions to colors, forms, and sizes.

1. *Hover box*, O; G, inclined planes of wire mesh leading from exits E, of the experiment box to the hover box.

2. *Illumination box.*—H, left compartment; P, right compartment; L, stimulus lamps; S, metric scales.

3. *Experiment box.*—A, entrance compartment; C, discrimination compartment; B, gateway between A and C; D and D′, electric passageways; N_1, N_2, N_3, glass areas for illumination in the sliding frame F; M and M′, cardboard shutters for closing exits E; U and R, electric keys for separate control of lamp in O and reading lamp (not represented in drawing) on outside of A, above R; K, stimulus key; I, inductorium.

The figure is symmetrical, hence the letters G, E, L, and S designate both the parts which they respectively mark and the duplicates of these parts on the opposite side of the figure.

lid J, containing the source lamps for the stimuli; and (3) the experiment box, divided into the two compartments, A and C. The illumination box and the experiment box were separated by a sliding frame F, containing three openings each 12 x 12 cm., in which were mounted the desired screens, N_1, N_2, N_3.

The point of the partition separating the electric passageways D and D′ was 31 cm. from the nearer ends of the metric scales.

In some respects a different adjustment of the apparatus from that used by Cole was employed by the writer. A 2 c. p. electric lamp instead of a 16 c. p. was located in the hover box. The inclined planes G, which Cole discarded, had been satisfactorily used throughout the present investigation. It may be more natural for a chicken to run up hill than down, as Cole intimates, but it is not quite so clear why the reader is asked, in explanation of the difficulty of descent, "to imagine a man descending a steep incline with his body leaning far forward." The difference in the results with this device is no doubt partly explained by the dark-room conditions maintained throughout the present test. The animals were attracted to the lamp in the hover box and did not wander off into the darkness after they reached it.

The source lamps in the illumination box were two turnip-shaped reflectors, having the lower half of the bulb frosted and the upper half mirrored. When operated at 110 volts, the end-on candle power, as measured at a distance of 3.048 meters, was 35.4 and 39.4 for no. 1 and no. 2, respectively, at the termination of the experiments. These measurements were made by the Electrical Testing Laboratories, 80th Street and East End Avenue, New York City.

The feet of the chicks were kept damp by a wet pad of cloth in the gateway B. This condition of the feet of the chicks insured greater regularity of the shock upon contact with the electric wires.

The experiments were all conducted in a dark-room. The apparatus within and without was painted a dull black. As a further precaution the discrimination compartment C, of the experiment box, was covered with a black cardboard hood so arranged that an opening above B was left of sufficient size to permit the experimenter to observe the behavior of an animal in the presence of the stimuli. The black cardboard shutters M and M′ were kept closed over the exits E until the animal had selected one or the other of the two stimuli. Thus the only light that reached a chick while in the act of discrimination was transmitted through the glass screens mounted in the sliding frame F. In the size and form experiments these screens consisted each of two plates of flashed opal glass, depolished toward the lamps, glossy-surfaced toward the chick. The illuminated areas of various sizes and shapes were produced by black mats of tin or cardboard that contained the appropriate openings and were set in the frame over the glass on the side away from the lamps. In the color work colored glass was inserted. The frame was adjusted on brass rollers that were constructed so as to work noiselessly. The lamps in the illumination box, mounted as they were on moveable cast-iron bases, could be shifted back and forth along the metric scales to provide such variation in the relative brightness of the stimuli as the demands of the experiment dictated.

The dark-basket mentioned in the following account was a willow basket lined with black cloth and covered with the same material. It was used for the purpose of dark-adapting the chicks. The animals were kept in this receptacle before and between trials during all the form and size work, and during all the color work except the preliminary tests.

METHOD

A chick that was being tested was introduced into the apparatus by hand at A, whence it passed through the gateway B, and walked toward one of the two lighted areas. If an error was made, the animal received an electric shock, controlled at K by the experimenter, whereupon it soon learned to pass to the other side of the compartment and secure its release. If a correct selection was made, the shutter M or M′ was withdrawn, exit E was thereby slightly illuminated with the dim light reflected from the hover box, and the animal found its way down the inclined plane G to heat, food, light, and water. There was a possible source of error in the method at this point against which the operator had to take precaution constantly. He made it a rule not to withdraw the shutter until a chick was well onto the wires in a position to be shocked. Occasionally a chick would stand near the edge of the electric wires and crane its neck toward the exit. It is easy to see how a slight noise on the proper side or a slight movement of the proper shutter might have precipitated a correct reaction when no discrimination of the important stimuli had taken place. The method was open to improvement at this point. So far as possible, chances for the involuntary interference of the experimenter should be eliminated. The writer was at work on an automatic device to meet this difficulty at the time these experiments were brought to a close.

The tests were given in series of ten trials each. In each series there was an equal distribution of the two stimuli on the right and left sides of the apparatus. These changes of position were handily produced by shifting the sliding frame, which contained like mounts at either end and the one to which these were opposed in the central opening. It is readily seen how a shift of the frame altered the relative positions of the two stimuli as well as changed the source lights for each.

The order of procedure was preference tests, training tests, and control tests. The first consisted of one or more series in which the chosen stimuli were alternated right and left and no electric shock was used. In the color work certain "preliminary" tests were made which were really of the nature of preference tests on various combinations of stimuli. In the training tests the right and left shifts were equally balanced but varied in order. In the control tests the shock was dispensed with.

REACTIONS TO COLORS

In the previous color tests, where the reflection method was used and black and blue were opposing colors, chicks not only formed the habit of rejecting blue but continued this rejecting reaction both when white was substituted for black and when a different tint of blue was substituted for that used in the original training. It seemed very probable that difference in color was the basis of discrimination. The experiment reported below was an attempt to test the color discrimination of the chicks by the transmission method. The colors chosen for this experiment were red and blue. During the course of the preliminary tests the following setting of the screens was adopted for the preference tests and the later training: Two red screens, one at either end of the sliding frame, each composed of two plates of red glass cut from the same sheet; one blue screen, in the middle of the frame, made up of three plates of blue and violet glass. These color names represent simply what the colors appeared to the experimenter to be in advance of spectroscopic analysis. In each of the three screens the colored plates were covered on the side toward the chicks with a plate of opal glass, and over this was set a black mat with a circular opening 8 cm. in diameter. A spectroscopic analysis of the light transmitted by these screens was kindly made for the writer by Professors Yerkes and Cole, which showed the range of red to be from 760 to 640$\mu\mu$, and that of the blue-violet from 480 to 430$\mu\mu$.

That young chicks are positively phototropic was suggested by their crowding toward the light shortly after hatching in the incubator. The same tendency was indicated also by preliminary tests 4 and 5 (see table 1), in which no. 65 markedly preferred the brighter of two non-chromatic stimuli. When red and blue were approximately equal in brightness for the human eye, red was preferred by the chick. See preliminary tests 1–3. Even when red was considerably less bright than blue, as judged by the human standard, the chicks still selected red in preference to blue. After no. 65 had selected the dim red in series 6 in preference to the comparatively bright blue, series 8 and 9 were arranged to discover whether the animal was exhibiting either a negative reaction to blue or a positive reaction to darkness. The result showed clearly that neither of these possibilities obtained. Blue 10 was preferred to darkness, 9–1. But when the distance of the source light from the color screen was increased from 10 to 80 cm., the preference of the chick for blue as against darkness was only slightly evident. Again, when the stimuli were red 100 and blue 10, no. 58 and no. 65 each selected blue nine times, red once. When, with no. 58, blue was kept constant at this intensity and red was gradually increased in brightness to 40, the preference was as gradually shifted to red. A like result was found in the tests on no. 65 immediately following. Thus, while the chicks had a natural preference for the brighter of two non-chromatic stimuli, the same could not be said of chromatic stimuli as humanly estimated. The peculiar preference for red when opposed to blue of much greater brightness value pointed to one or both of the following conclusions: (1) A difference exists between the chromatic luminosity values for the chick and those for the human; (2) chicks exhibit a qualitatively determined preference, such as is apparent in some other animals. Since, however, the preference of the chicks for red and blue varied with the variations in the relative brightness of the two color stimuli, brightness was clearly shown to be a controlling factor. Whatever natural preference the chicks might have had for red, this was overcome, in every case tried, by a sufficient relative increase in the intensity of the opposing blue. In view of these facts it was thought best at this stage of the investigation to seek such intensities of red and blue that initial preference for neither would appear. This point of indifference once located, it was planned to proceed with a process of training to determine the ability of the chicks to discriminate between two such red and blue stimuli. In the final preliminary tests red, from a source light 60 cm. distant from the screen, with a blue from a source of about equal power, 10 cm. distant, gave the reaction records of 6–4 and 7–3 for chicks no. 58 and no. 65. Red 70 with blue 10 resulted 4–5 and 3–7 for no. 61 and no. 65, respectively. Hereupon, the apparatus was slightly modified in preparation for the more careful preference tests that preceded training on the indifferent combination such as red 70, blue 10 seemed to approach. Cardboards containing circular openings 8 cm. in diameter replaced those with rectangular openings used in the preliminary tests. Circular water screens (Bausch and Lomb projection-lantern cooling cells) 12.5 cm. in diameter and 4.8 cm. thick were substituted for the rectangular cells in use up to this time. Furthermore, during the preference tests and throughout the following training and control tests, provision was made for the dark-adaptation of the animals by keeping them in the dark-basket in the dark-room for a period of ten minutes each day just prior to experimentation. Between trials also each chick was kept in the dark-basket. Among the preference tests, A–F, with red 70 opposed to blue 10, red was selected by the four chicks 28 times, blue 52 times, out of a total of 80 trials. Red and blue at these intensities, although not ideally balanced, were weighted on the proper side for a test of the chicks' ability to form a habit of positive reaction to red, so training to this end was begun. The habit proved a comparatively easy one for the chicks to form, as evidenced by the first training series, 1–8. Each of the four animals finished the training, including the final twenty perfect reactions, within a total of 80 trials. When all the chicks had formed the red-blue habit perfectly, a slightly different intensity of blue was opposed to red at 10 and 100 alternately. The chicks continued the specific reaction to red, two of them without error. They had evidently not formed a habit of specific reaction to red or blue of a particular brightness value. As soon as the original habit was ascertained to be in perfect condition in all the chicks, they were tested further with red and blue, each color this time being used equally often at the value of 10 against the other color at 100 in a series of ten trials. Surprisingly, not a single error was made in this series by any chick. It is well to bear in mind that blue 10 was without question much brighter than red 100; and red 10 than blue 100.

Elsewhere[3] the writer has said something about Yerkes' discrimination method and the peculiar emphasis that sometimes attaches to the stimulus in connection with which the electric shock is administered. This emphasis on the blue was very noticeable in the results from all four chicks. When white light was opposed to blue in the control test, the chicks uniformly rejected blue, regardless of which stimulus had the greater brightness value. When white was substituted for blue and the factor of brightness eliminated by variation, the score was red 29, white 27, in a total of 56 trials. No. 61, however, with a little training formed a perfect red-white habit, the element of brightness being again eliminated in the manner just mentioned.

What shall be our interpretation of the above facts? When the stimuli were red 70 versus blue 10 in the preference tests, no marked preference was shown for either stimulus. Preference shifted decidedly to blue in the combination red 100 versus blue 10, and just as decidedly to red in the combination red 40 versus blue 10. That is, from the indifference point preference varied as the relative brightness of the stimuli varied. It seems reasonable to suppose, therefore, that at the above indifference values, the two stimuli were approximately equal in brightness for the chicks, provided brightness alone was the determining factor in the preference. The readiness, however, with which the chicks came to discriminate these two stimuli leads one to suspect that the basis of the discrimination was not difference in brightness. But this result alone is not proof conclusive that color was a factor. A point of brightness indifference may not mean brightness equality, nor need brightness equality for the chick in advance of training mean brightness equality after training. The selective reaction might therefore have been based on difference in brightness. But the evidence that color was a factor in these reactions does not rest on the fact alone that the indifference stimuli were discriminated. The fact that blue of different brightness value from that used in the training was rejected consistently when appearing with red that was not the same in brightness as the original red, ruled out the probability of specific reaction to specific brightness values. And more important still was control test 5. When red was increased in brightness from 70 to 10 and the accompanying blue decreased from 10 to 100, blue was rejected. When red was decreased from 70 to 100 and used with blue 10, blue was again rejected. Alternations of these combinations failed to interrupt the specific rejection of blue. It should be borne in mind that red 10 was brighter than blue 100 both for the chick (see preference test C) and the human; and red 100 was less bright than blue 10 both for the chick (see preference test F) and the human. Finally, in the control test on white-blue, the rejection of blue continued perfectly when white was much brighter and when it was much less bright than blue. Thus it seems almost certain that the quality of the stimulus as well as quantity played a determining part in the reactions of these animals.

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[3] *Loc. cit.*, p. 69.

An attempt was made to photometer the stimuli with a Lummer-Brodhun photometer and a Hefner amyl acetate lamp, but without success, primarily on account of the very low intensities of the weaker stimuli. But even had the intensities of the colored lights been sufficiently high for examination by this method, limited progress would have been made toward an objective measurement of the color values, for this method depends upon a human judgment such as we have already given. Radiometry is the hope of the experimenter in this difficulty.

TABLE 1

Red-Blue Reactions

Chicks hatched Oct. 26, 1908

Series	Date	No. 58 Sex, F. Red	No. 58 Sex, F. Blue	No. 61 Sex, M. Red	No. 61 Sex, M. Blue	No. 62 Sex, M. Red	No. 62 Sex, M. Blue	No. 65 Sex, M. Red	No. 65 Sex, M. Blue	Remarks
					Preliminary Tests					
1	Nov. 17			10	0	10	0			Both lamps at 40
2	" 18			10	0	9	1			Both lamps at 40
3	" 19					10	0			Both lamps at 40
								Btr.	Dkr.	
4	Nov. 18							8	2	Lamps at 20 and 70, varied r and l. Opal glass.
5	" 19							10	0	
								Red	Blue	
6	Nov. 20	10	0	10	0			10	0	Right lamp, 10; left, 80.
7	" 20					10	0			Right lamp, 20; left, 70.
								Dk.	Blue	
8	Nov. 24							1	9	Darkness; B, 10.
9	" 24							4	6	Darkness; B, 80.
								Red	Blue	
10	Nov. 25	1	9					1	9	R, 100; B, 10.
11	" 25	0	6							R, 50–80; B, 10.
		3	1							R, 40; B, 10.
12	" 25							5	0	R, 40 and 50; B, 10.
								2	3	R, 60; B, 10.
13	" 26	6	4					7	3	R, 60; B, 10.
14	" 26			4	5			3	7	R, 70; B, 10.
					Preference Tests					
A	Nov. 28	6	4	1	9	3	7	4	6	R, 70; B, 10.
B	" 30	5	5	1	9	3	7	5	5	R, 70; B, 10.
C	Dec. 1	9	1	1	9	9	1	9	1	R, 40; B, 10.
D	" 1	0	10	0	10	0	10	5	5	R, 100; B, 10.
E	" 2	10	0	3	7	8	2	9	1	R, 40; B, 10.
F	" 2	3	7	1	9	1	9	5	5	R, 100; B, 10.
					Training Tests					
1	Dec. 4	5	5	4	6	5	5	5	5	R, 70; B, 10. Sk, 6
2	" 5	5	5	8	2	7	3	6	4	R, 70; B, 10. Sk, 6.
3	" 7	9	1	7	3	6	4	7	3	R, 70; B, 10. Sk, 6.
4	" 8	9	1	8	2	6	4	9	1	R, 70; B, 10. Sk, 6.
5	" 9	10	0	8	2	10	0	10	0	R, 70; B, 10. Sk. 6.
6	" 10	10	0	8	2	8	2	10	0	R, 70; B, 10. Sk, 6.
7	" 11			10	0	10	0			R, 70; B, 10. Sk, 6.
8	" 12			10	0	10	0			R, 70; B, 10. Sk, 6.
					Control Tests					
1	Dec. 13	10	0	7	3	8	2	10	0	R, 10 & 100 alt.; B, 20.
2	" 14	9	1	10	0	10	0	10	0	R, 70; B, 10. Sk, 6.
3	" 14	9	1							R, 70 B, 10. Sk, 6.
4	" 14	10	0							R, 70; B, 10. Sk, 6.
5	" 15	10	0	10	0	10	0	10	0	R & B, 10 & 100, alternately.
6	" 16	8	2					9	0	R, 70; B, 10.
7	" 16			8	0	6	4			R, 7–100; B, 7–105.
				1						R, 100; Darkness.
					1					Darkness; B, 10.
8	" 17	6	0	6	0	5	1	6	0	R, 70; B, 10.
		White	Blue	White	Blue	White	Blue	White	Blue	
9	Dec. 17	10	0	10	0	10	0	10	0	B=, <. and > W in brightness
		Btr.	Dkr.	Btr.	Dkr.	Btr.	Dkr.	Btr.	Dkr.	
10	Dec. 18	2	6	8	0	6	2	5	3	Both sides W. Opal glass.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
11	Dec. 21	4	0	4	0	4	0	4	0	R, 70; B, 10.

TABLE 1—*Continued*

Series	Date	No. 58 Sex, F.		No. 61 Sex, M.		No. 62 Sex, M.		No. 65 Sex, M.		Remarks
		Red	White	Red	White	Red	White	Red	White	
					Control Tests—*Continued*					
12	Dec. 21	8	6	10	4	4	10	7	7	R, btr. 6 trials; W, btr. 6; eq. 2.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
13	Dec. 21	5	1	4	2	4	2	4	2	R and B varied in brightness, now one, now the other brighter, with extreme diff's.
		Red	Blue	Red	Blue	Red	Blue	Red	Blue	
14	Jan. 4	4	0	4	0	4	0	4	0	R, 70; B. 30.
		Red	White	Red	White	Red	White	Red	White	
15	Jan. 4	5	5	8	2	6	4	5	5	R, 6–30; W, 15–105.
					Training Tests					
1	Jan. 5			9	1	5	5			R, 6–30; W, 15–105.
2	" 6			8	2	9	1			R, 6–30; W, 15–105.
3	" 6			9	1	8	2			R, 6–30; W, 15–105.
4	" 7			10	0	9	1			R, 6–30; W, 15–105.
5	" 7			10	0	8	2			R, 6–70; W, 70–105.*
6	" 8					6	4			R, 6–70; W, 70–105.
7	" 8					7	3			R, 6–70; W, 70–105.
8	" 8					7	3			R, 6–70; W, 70–105.
9	" 9					8	2			R, 6–70; W, 70–105.
										Note: No. 62 large, slow, and inactive from start. At time of last test physically weak.

* R, 10 > W, 100 in brightness.

Explanation of abbreviations in table: R, red; B, blue; W, white; Sk, electric shock; Dk, darkness; Btr, brighter; Dkr, darker; Eq, equal.

A numerical value appearing with the name of the stimulus represents distance in centimeters of the source light from the screen.

REACTIONS TO FORMS

As a test of form discrimination chicks were afforded an opportunity to select a circle when appearing along with a square. For this experiment the sliding frame of the apparatus was fitted with three black tin plates, in two of which had been cut square openings each with a side of 4.43 cm.; in the third and central one was cut a circular opening of the same area, its diameter being 5 cm. Compactly behind each of the form plates were mounted two plates of opal glass, each pair of plates having an aggregate thickness of about .515 cm. The lamps were stationed at a distance of 90 cm. from the screen. The Bausch and Lomb projection-lantern water cells were adjusted immediately back of the opal glass plates, on the side of the source lights. With the above arrangement white lighted areas with sharp outlines were presented to the animals, with variations in brightness, size, and position under good control.

Three chicks, nos. 73, 76, and 87 were used in these tests, one of which yielded positive results. After 60 training tests, no. 73 was unfitted for further work by a misuse of the electric shock. When no. 87 had completed 160 trials in the training, the work was discontinued on account of the poor physical condition of the animal. Starting with preference trials of 9 reactions to the circle and 11 to the square, its total number of reactions to the circle during the training was 101; to the square, 59. The final two series registered 9–1 and 8–2 in favor of the circle. There were thus indications that the training had been somewhat effective.

The results from no. 76, on the other hand, were more convincing. Reacting indifferently in the preference tests to the circle and the square of equal area and brightness, it was able to select the circle 38 times out of the last 40 trials in a training series of 120 trials. When the positions of the source lights were shifted from 90 to fixed positions at 80 and 100 cm. respectively, so that the forms were illuminated unequally, no. 76 selected the circle 18 times out of 20 trials. Training was then resumed for two series with the forms of equal brightness, after which the sliding screen was mounted from left to right as follows: 5 cm. circle, 4.43 cm. square, and a circle equal to that circumscribed about the 4.43 cm. square. Ten trials with this combination netted 8 selections of the circles and 2 of the square. When, however, the next change in the forms was made, there appeared to be no sign of discrimination. The corresponding inscribed circle was substituted for the circumscribed, accompanying as in the previous test the square and circle equal to each other in area, and the first ten reactions recorded were 5 for the circles and 5 for the square. An attempt was now made to train the chick to select these circles in preference to the square. After this training had been prolonged for 70 trials, the chick was selecting the circles as against the square 9–1. At this point the frame was set with the circumscribed and the inscribed circles in combination with the square, whereupon the chick made the record 10–0. For the series following this one, conditions remained the same except that the lamps were exchanged in the illumination boxes and a different tin plate was used to produce the square form. The series resulted 9–1. When this series was repeated with a different order of shifts of the frame, the animal made the record 10–0. For data see table 2.

TABLE 2

Circle-Square Reactions

Chick No. 76. Sex, F.

Hatched December 3, 1908

Series	Date	Right	Wrong	Remarks
A	Jan. 21	4	6	Cir. and Sq. equal in area and brightness.
B	“ 22	5	5	“ “ “
1	“ 23	6	4	“ “ “
2	“ 24	6	4	“ “ “
3	“ 25	2	8	“ “ “
4	“ 25	7	3	“ “ “
5	“ 25	10	0	“ “ “
6	“ 26	6	4	“ “ “
7	“ 26	9	1	“ “ “
8	“ 26	5	5	“ “ “
9	“ 26	9	1	“ “ “
10	“ 26	10	0	“ “ “
11	“ 26	10	0	“ “ “
12	“ 26	9	1	“ “ “
Control Tests				
1	Jan. 27	9	1	Lamp, r, 80; lamp, l, 100.
2	“ 27	9	1	Lamp, r, 100; lamp, l, 80.
Training Tests				
13	Jan. 30	7	3	Cir. and Sq. equal in area and brightness.
14	“ 30	9	1	“ “ “
Control Tests				
3	Jan. 30	8	2	Sq. vs. equivalent and circumscribed circles.
4	“ 30	5	5	Sq. vs. equivalent and inscribed circles.
Training Tests				
15	Jan. 30	8	2	Sq. vs. equivalent and inscribed circles.
16	“ 30	6	4	“ “
17	“ 30	7	3	“ “
18	“ 30	8	2	“ “
19	“ 30	9	1	“ “
20	“ 31	7	3	“ “
21	“ 31	9	1	“ “
Control Tests				
5	Jan. 31	10	0	Sq. vs. circumscribed and inscribed circles.
6	“ 31	9	1	Same, lamps exchanged. New Sq. form plate.
7	“ 31	10	0	Same, different order of stimuli.

After negative results in several experiments on forms, the results obtained with no. 76 are interesting. I believe I was fortunate in the choice of animal for this work. My notes describe this chick as rather undersized, strong, active, sensitive, cautious, and quiet. Detailed records of its reactions show that selection of stimuli was made by affirmation and negation most frequently, by comparison occasionally.

REACTIONS TO SIZES

The method of testing size discrimination was similar to that followed in the tests on forms. Two circular areas of different size were presented to the chicks, one 5 cm. in diameter, the other 8 cm. In the sliding frame of the apparatus an 8 cm. circle was mounted at each end, a 5 cm. circle in the middle. In other respects the appointments of the apparatus were the same as in the form experiments.

In these tests three factors were taken into consideration, any one of which might have become a means of discrimination:

1. Difference in size of the lighted areas,
2. Difference in brightness of the lighted areas, and
3. Difference in brightness of the right and left sides of the experiment box.

To eliminate other means of discrimination but the difference in size of the lighted surfaces, the following list of tests was adhered to throughout the training:

1. Sides of box equal in brightness: S, 60, r; L, 96, l
2. Sides of box equal in brightness: S, 60, l; L, 96, r
3. Circles equal in brightness: S, 96, l; L, 96, r
4. Circles equal in brightness: S, 96, r; L, 96, l
5. Large circle the brighter: S, 80, l; L, 40, r
6. Small circle the brighter: S, 40, r; L, 80, l
7. Large circle much the brighter: S, 100, l; L, 30, r
8. Small circle much the brighter: S, 30, r; L, 100, l
9. Sides of box equal in brightness: S, 60, r; L, 96, l
10. Circles equal in brightness: S, 96, l; L, 96, r

Explanation of abbreviations: S, small circle; L, large circle; numbers, distances in centimeters of source lights from screen; r, right; l, left.

The experiment was undertaken with chicks no. 70 and no. 83. The preference tests showed a marked inclination of the chicks to react positively to the larger or negatively to the smaller of the two circles, each animal reacting 8 times to L and only 2 to S. The plan of the work was interfered with somewhat by the necessary retirement of no. 70. After 120 trials this animal was unable to continue on account of physical debility, an unfortunate failing of these laboratory chicks after a given period. No. 70, however, showed signs of acquiring the small-large habit, for in its last 50 trials there were 38 reactions to S and 12 to L. No. 83 remained in the experiment with the positive result shown in table 3, which follows:

TABLE 3

SMALL-LARGE REACTIONS

Chick No. 83. Sex, M.

Hatched December 12, 1908

Series	Date	Right	Wrong
A	Jan. 13	2	8
1	" 14	2	8
2	" 15	3	7
3	" 16	5	5
4	" 18	5	5
5	" 19	6	4
6	" 20	3	7
7	" 21	7	3
8	" 22	8	2
9	" 23	5	4
10	" 24	5	5
11	" 25	6	4
12	" 27	4	6
13	" 27	7	3
14	" 27	5	5
15	" 27	6	4
16	" 27	9	1
17	" 27	6	4
18	" 28	8	2
19	" 28	8	2
20	" 28	9	1
21	" 28	8	2
22	" 28	10	0
23	" 28	10	0
24	" 28	10	0

SUMMARY AND CONCLUSIONS

The chicks in advance of training selected the brighter of two non-chromatic stimuli.

According to the human standard of brightness values, a similar law did not hold for chromatic stimuli. This exception points in the direction of one or both of the following conclusions:

(1) A difference exists between the chromatic luminosity values for the chick and those for the human;

(2) Chicks exhibit a qualitatively determined preference, such as is apparent in some other animals.

Brightness was found to be a controlling factor in the responses to optical stimuli.

All four chicks after brief training formed the habit of selecting without error one of two colors at the brightness-indifference point.

This selective reaction was not determined by red or blue of particular brightness values; neither was it determined by the relative brightness of the two stimuli.

Thus seemed verified a conclusion of our earlier work, namely, that quality as well as quantity of the optical stimulus played a determining part in the reactions of these animals.

Positive reaction to a given stimulus as indicated in a table of reactions did not necessarily imply specific reaction to that stimulus. It sometimes meant merely a specific rejection of the opposing stimulus.

A chick learned to discriminate two optical stimuli on the basis of difference in form.

A chick learned to discriminate two optical stimuli on the basis of difference in size.

NOTES

BEHAVIOR OF THE YOUNG BIRD IN BREAKING OUT OF THE EGG

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The older naturalists, especially Réaumur, made some careful observations on the behavior of chicks in breaking out of the egg. Their observations are summarized in one[1] of Rennie's excellent little books. The main facts are these. The chick chips the egg a little at a time with its bill, and as it does so it turns around inside the egg-shell, the axis of its rotation being precisely the long axis of the egg. The consequence of this turning is that the tip of the bill, chipping the shell as it goes, describes a very exact circle around the large end of the egg, and thus the large end of the shell is cut off. The time which it takes to finish the cutting is very variable. When the opening has been extended nearly or quite around the egg, the chick pushes in such a way as to separate the two sections of the shell and tear any shreds of membrane which may have been holding them together. Thus the little bird effects its escape.

From the fact that pigeons' eggs are invariably opened by a cut which neatly encircles the large end, we should infer that the young pigeon opens its egg in the same manner as does the young chick. And this conclusion is confirmed by the following observations on two young doves (Turtur risorius). The accounts are transcribed from my notes.

"No. 46, April 19, 1911. 8:30 A. M. My wife found egg chipped and called me to see it. While we watched it, the young chipped the egg about one-third the way round (120°) in about ten minutes. The young made a strong movement, then rested for several seconds, then made another strong movement, and so on.[2] Each movement seemed to consist of (1) Pushing out the large end of the egg, i.e., pushing lengthwise of the egg; (2) thrusting the bill through the shell, the bill coming just far enough to break through, in fact many times not breaking through but only bulging the shell; (3) turning round a few degrees. We could see this turning through the hole in the shell; also we could see that each thrust of the bill appeared a little beyond the previous one. With the egg large end up, the turning was anti-clock-wise.

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[1] [Rennie, James. Published anonymously.] The Domestic Habits of Birds. London, Charles Knight, 1833.

"Not wishing to sacrifice the life of this young bird, I replaced it in nest under parent. 9:45 A. M. We look and find young out of shell."

"No. 51, August 28, 1911. Just after 2 P. M., I put seed in the pen, and sitting bird comes off the nest to feed. I see that egg is chipped almost all the way round, and soon the young bird makes intense struggle to get out. It makes a strong effort, and then rests, breathing deeply and rapidly, about eight or ten seconds, then makes another strong effort. Once, apparently more exhausted than usual, it rested nearly forty seconds. After several such movements it got the halves of the shell separated at an angle (they still held together at one point). During the next few struggles it sometimes seized edge of shell with one foot and apparently tried to push it away. But the final dislodgment of shell seemed to be made by powerful movements of head and neck. I was surprised at the violence of the movements.

"Having got out, it rested on ventral surface. When touched, moved the head a little, but seemed unable to raise head from nest."

The question arises: By what means does the little bird turn around inside the egg-shell? When I asked Professor Whitman this question, he answered thus: "The bill itself may pull or push the embryo round in the egg. Yet I think the turning may be done by the feet. I think it would not need much force. I have taken an egg between my finger and thumb, just before hatching, and have felt a bumping inside; I feel very sure that this was not imagination. The bumping may have been caused by the bill, but more probably by the feet. The little fellow is undoubtedly in a very uncomfortable position, with the head pushed between one wing and leg, and this is what makes it move and thrust the bill against the shell of the egg. The opening and closing of the bill, which is seen in the young pigeon within the egg, is supposed to be due to gasping for air." To determine whether the chick turned itself round by its bill or by its feet, Réaumur tried the experiment of breaking away the shell in advance of the bird's bill, so that the bill had nothing to push against; "the consequence was, that each chick was sooner released from confinement than if it had had to effect its own exit." From which, Réaumur concluded that the turning was probably performed by means of the feet.[3]

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[2] Similar rhythmic activity was noted by Breed in the chick. Breed, F. S. The Development of Certain Instincts and Habits in Chicks. *Behavior Monographs*, No. 1, Boston, 1911. See pp. 5–7.

This general method of breaking the shell, which we have seen to be common to the domestic fowl and pigeons, Réaumur found to hold also for ducks, and he thought it probable that it was common to all birds.[4] Observations of the hatching activity of wild forms are, naturally, rare.[5] The indications are, that the type of behavior which we have described is common to most if not all the Carinatae. But in the ostrich, although the posture of the young within the egg[6] much resembles that in the Carinatae, there are some considerations, one of which is the extremely hard egg-shell, which lead one to suspect that the mode of exit of the young ostrich must differ from that of the young carinate bird.

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[3] Quoted in Rennie. *Op. cit.*, pp. 167–168.

[4] Rennie. *Op. cit.*, p. 171.

[5] I have happened upon only two, each recorded all too briefly. Hudson, W. H. The Naturalist in La Plata. London, 1892. Page 112 records observed hatching of the Jacana. Moore, R. T. The Least Sandpiper During the Nesting Season in the Magdalen Islands. *The Auk*, Vol. 29, N. S., April, 1912. Page 218 records observed hatching of this Pisobia.

[6] Beebe, C. W. The Bird, its Form and Function. New York, 1906. See figs. 370 and 371.

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OBSERVATIONS OF THE SENSES AND INSTINCTS OF THE RACCOON

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When an animal is free, observations of its behavior cannot usually be repeated in order to verify the observer's explanation of it. For example, Erasmus Darwin saw evidence of reason in the wasp when, as he averred, he saw a Sphex, with a prey too large for it to drag, cut off the wings, legs, and head of the prey, whereupon it easily transported the trunk and stored it. Fabre replied that Darwin had not seen a Sphex in this case but an insect which instinctively dismembers its prey and stores only the body. Recently one of my students reported that he had seen a hornet do this.

With observations so rare and isolated as these it will surely be long before different observers will agree in their interpretation of the behavior. Consequently, I report in what follows only observations of the raccoon which were made repeatedly. It may still be objected that the conditions were artificial. Yet when several raccoons are set free from captivity for an hour or two each day and their behavior noted the conditions appear so nearly natural that behavior observed in all the individuals would seem to be typical for this animal.

THE SENSES

Touch: The most conspicuous behavior of the raccoon seems to be associated with the sense of touch, which is highly developed in the palm of the forepaw and the tip of the nose. During their hours of activity the animals were most often busy in exploring with their paws the floor and objects on the floor of the room in which they were kept. The cage was an object of similar exploration. When the cage was moved the uncovered floor space excited this reaction with renewed activity. Dark places, as your pocket or a knothole, are explored by touch hundreds of times. Observers agree that the motive for this exploration is instinctive curiosity and I shall discuss this behavior further in connection with that topic. Notwithstanding the strength of the raccoon in clinging and climbing, no touch is softer or more gentle than that of his forepaws when engaged in this investigating activity.

An evidence that the nose is sometimes used for pure touch is the fact that these animals frequently investigated the experimenter's hands, and even his face, with the nose. This also seemed to be an affair of pure curiosity and quite breathless while it lasted, just as a human being may not breathe perceptibly when intent on some object. In fact this momentary stopping of respiration is given by Meumann[1] as one of the most marked physical accompaniments of human attention. This use of the nose as an organ of touch was observed on the sixth day after we received the raccoons and frequently thereafter. Occasionally they would both touch a strange object with the nose and sniff at it also.

I was unable to detect that the vibrissae were used as organs of touch, and perhaps this would be difficult to do unless one could have a blind animal under observation.

Taste: The raccoon's taste for sweets is especially marked. All other foods were promptly deserted for cane sugar by my animals, though Brehm states that the raccoon prefers "flesh" to other foods. My raccoons avoided all food which had a purely sour taste, yet ripe apples and peaches were eaten which have for human taste a slightly acid tang along with the sweet flavor. Unlike herbivorous animals the raccoon refuses to taste salt. I do not know of any carnivore which will accept salt or which frequents salt licks. Consequently the raccoon's rejection of salt may be a general characteristic of flesh-eating animals. Beyond these statements we can only enumerate the foods which raccoons accept readily, which of course give complex tastes.

Next to sugar the raccoons preferred boiled beef and they were almost equally fond of uncooked apples, peaches, plums, and cherries. My animals never ate the raw beef which we offered them a few times. Some raccoons have been forced to eat it but they appear not to thrive on such food. My raccoons were often seen to catch and eat flies. They would eat grains of corn, even when dry and hard, if they were hungry. Bread made of either corn-meal or flour was readily accepted. It seems evident, therefore that the raccoon in his native haunts lives upon forest fruits and buds, and upon flies, beetles, minnows, etc.

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[1] Meumann, E. Vorlesungen zur Einführung in die experimentelle Padagogik. Bd. I, p. 159, 2d. Aufl. 1911.

Brehm [2] says, "In respect to his food the raccoon is a true bear. He consumes everything that is eatable, but seems to be a real epicure who prides himself on picking out the best morsels, if only it is possible. The most different kinds of fruits, as chestnuts, wild grapes, maize, so long as the ears are not hard; fruits of every kind furnish valuable means of nourishment; but he tries to catch birds and their broods, knows how to steal upon a chicken or a dove, catches nimble fish, crabs and testaceous animals, and ventures at ebb-tide, for the love of such feasts, far out into the water. Especially does he like to eat oysters and understands how to open them skilfully."

Smell: So far as I could observe the raccoons did not often employ the sense of smell, though this may have been due to their captive condition. In no case did they seem to find pieces of meat on the floor by means of smell. If one of them saw a small piece of meat dropped in the hay on the floor he would search for it carefully but beyond a distance of a few inches he did not seem to smell it. They found small pieces of loaf sugar on the floor quite as promptly as they did meat, yet from the standpoint of the human sense of smell sugar has no odor.

In one case smell was evident. When the animals were to be fed the basin of food was usually placed on the step while the door was being unlocked. During this time all of the raccoons sniffed noisily at the crack beneath the door. When it was opened, however, they *looked* for the food basin. So in this case smell was evident only when sight could not be used.

On receiving the fifth raccoon, which was very young, I placed it on the floor of the room in which the older animals were kept. They showed both fear and curiosity in regard to it. After a time they came within two feet of it, stretching their muzzles toward it with slight sniffing. Finally they touched its fur with their noses. The males then took no further notice of the young animal but the female touched it with her nose many times during the following half-hour.

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[2] Brehm, A. E. Thierleben, Erster Band, Leipzig, 1875.

Living as they do, mainly in trees, and securing their food chiefly by means of sight, these animals may need to depend but little on the sense of smell. Doubtless they are susceptible to odors coming through the air, as the smoke of forest fires, but the two cases above described are the only ones which I observed of the use of this sense. I believe that they have no gift of trailing other animals as dogs do, though it has been assumed that they have the power to do so.

Vision: The raccoon has a keen sense of sight. As in the case of Clever Hans, a movement of the experimenter so slight as to be unconscious on his part was promptly seen by the animals and responded to as a food signal. Consequently it was necessary to make the movements involved in giving the animal a morsel of food even when he was not fed. Otherwise he would respond to the unconscious movements preliminary to feeding him and cease to pay attention to the different forms or sizes of the cards to which we wished him to respond. Attention to the experimenter's movements must have been established very early for within a week after we began work with the young animals one of them climbed to the top of a barrel on which the bottle of milk had been placed. It was impossible that he could have seen the bottle so his climbing for it must have been due to his having seen it placed there.

Later all of the raccoons recognized the food basin by means of sight and developed great cunning in their attempts to get food from it.

Hearing: This appears to be the special protective sense of the raccoon. The slightest sound produced (1st) perfect immobility, and (2d) fear and scurrying to the highest part of their place of confinement. If the experimenter tried during this fear to reach up and take hold of one of the animals it would retreat and snap though a moment before it was eager to be fed and quite willing to be handled.

Every sound at a distance was listened to intently for several seconds after the experimenter had ceased to hear it. On one occasion all the raccoons became still and yet the observers could hear no sound. Investigation showed that a man was trundling a wheelbarrow over the grass plot at least 100 yards distant from the house in which the raccoons were kept.

The sound caused by dropping on the floor a piece of meat, one-half the size of a grain of corn, was often heard by each of the animals. They turned directly toward the source of the sound. Hence they not only hear faint sounds but localize them well. Localization was further tested by putting raccoon No. 3 in a large box with a solid back. The experimenter then scratched on the outside of the back of the box with a small stick. The raccoon turned directly to the spot. The place was changed some two feet. He turned instantly to the new place and grasped with both forepaws at the exact spot. He did this repeatedly. His behavior suggests that localization of sound is much more definite than that of the human ear. His grasping at the spot might indicate that the raccoon catches some small prey partly by the aid of hearing.

If a raccoon which is working for meat hears another munching loaf-sugar he will at once stop work and approach the other in search of sugar. In this case the animals seem to have a definite sound-taste association.

On the principle that animals which make sounds hear sounds we may, in connection with hearing, mention the sounds which the raccoon is capable of making. A warning growl always accompanied eating when they were fed. When hungry they sometimes emit a sound about midway between a whine and a purr, "a whimpering cry." This sound is well known to woodmen and is far more characteristic of the young than of the adult animal. It has been described as varying from the quavering note of a "screech owl" to a sound like a colt's whinny, though I can detect no resemblance to the latter sound. It does vary from a sort of contented purr in the young to a rougher, higher pitched, whining sound when the animals are very hungry or when, in the case of my raccoons, they were trying hard to overtake the experimenter who was walking too rapidly for them to keep up with him. When forcibly held their whining and growling is somewhat similar to that of a dog. In fighting the animal gives short, sharp barks as he snaps.

Turning once more to the whining-purr, there is less and less of it (in captivity) as the animals grow older and only long waiting for food produces it. In the forest it may be used as a call to others. A very young raccoon, making this cry from loneliness or in search of its mother, will cease to make it if gently stroked or scratched.

Davis [3] describes the mother's warning note to the young which, doubtless, I have not heard.

INSTINCTS OF RACCOONS

I have already described the instinctive cry of the raccoon. It subsides into a whining purr of satisfaction as hunger is allayed. A young animal will drop off to sleep while quietly whimpering.

Sucking: The impulse to suck is ripe at birth, of course, and with it is a tendency to press with the forepaws on the breast of the mother. When a young raccoon is fed from a bottle the two forepaws are gropingly outstretched, then brought together and clasped just as a human babe sometimes clasps its hands without interlocking the fingers. Thus clasped they sink together down to the floor only to be outstretched again in blind and futile groping. The act is part of the sucking reflex and its persistence shows its instinctive character. The sucking impulse remained active until the animals were (as estimated) four months old. This is probably somewhat longer than its normal period. A slight tendency to lap milk appeared in the twelfth week. Hunger would have brought them to this mode of feeding much earlier. The sucking instinct may be perverted, the animal sucking parts of its own or its companion's body. This perversion waned, however, with the waning of the instinct and disappeared with it.

Creeping: The young raccoon seems to pass through a stage of creeping. The youngest one that I had under observation was not more than a month old when he was received. His movements over the floor were so different from those of the adult that they may be described only by the word, creeping. The forelimbs are widespread, the palms outstretched as if feeling the way. The body is propelled, therefore, chiefly by the hinder limbs which are wider spread than in the later walking. The whole movement is uncertain, staggering and tremulous, and frequently results in rolling over on the side. Hence, it is suggestive of creeping though it may be, instead, the beginning of the impulse to climb.

Climbing: This instinct involves the sense of support, which is present before the raccoon possesses either the strength or the muscular co-ordination necessary for climbing, and the impulse to cling to any support. The sense of support is best described by an example. When raccoon No. 5 was probably not more than two weeks old I placed him one day upon the top of a small closed box six inches high. He groped over the top of this box in the manner which I have described as creeping, with his forepaws extended, feeling the way. But the moment his paws felt the edge of the box the animal shrank back and began to grope in another direction. Again he would find the edge and again shrink back and start anew. Apparently at this age vision did not serve to show him that he might safely drop to the floor. It seems likely that this impulse enables the young raccoon to remain safely in a high nest, even though it were not enclosed. At any rate its advantage to a tree inhabiting animal is obvious.

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[3] Davis, H. B. Amer. Jour. of Psych. 1907, Vol. 18, pp. 454.

When the raccoon is a month old it is able to sustain its weight by clinging to a support by any one of its paws and this it does instinctively. It will hang thus from a rod or the experimenter's finger, unless it can feel a support beneath it. In the latter case it instantly releases its grasp and drops to the secure footing. One of the chief difficulties in handling raccoons is this instinctive tendency to cling to all nearby objects. In being put into a cage through the door the animal will lay hold of the sides with his paws and the difficulty encountered in disengaging him shows his remarkable strength.

As soon as the young raccoon can walk well any bush or tree arouses his impulse to climb. At first there is some awkwardness and two of our animals were seen to fall from a small tree, when about eight weeks old. A little later they could hardly be dislodged at all. After the animals ceased to follow the experimenter when set free they always ran to climb the nearest tree. As Brehm states, the raccoon often climbs along a branch with his back down "like a sloth or an ape." When the same writer says that the raccoon makes "leaps from one bough to another with unfailing certainty," his observations differ from mine. Instead of this my raccoons always laid hold of one bough before releasing the other. They go from one bough to another very quickly yet they rely much more on their strength in clinging than on their agility. In fact I tried to get the animals to leap from one box to another for food but they usually clung to the first box with their hind paws and, with only a sort of half leap, reached the second with their forefeet and pulled themselves upon it. Their climbing up the trunk of a tree is exactly like that of a bear.

Play: The raccoon is everywhere described as very playful. One would sit for a long time and play with his hind feet or the tip of his tail. Three were observed to play in this fashion for one and a quarter hours, with almost no pause. While my animals had to work twice a day for their food I observed only momentary play, or perhaps curiosity, as the tendency to pick up a straw or bit of cornhusk and roll it for a moment between their forepaws. In some degree, therefore, their play seems to depend on the possession of surplus energy. When well rested they played roughly with each other in mock fights, running and seizing each other gently with the teeth, rolling over and over in their tussles. In this play they would often climb to the shoulder of the observer, whereby he may learn both the strength of their grip and the sharpness of their claws. They also make pretense of biting your hand in play, a characteristic reaction of the pet raccoon.

Beckmann says, "In the numberless leisure hours which every captive raccoon has he does thousands of things in order to dispel the tedium. Now, he sits upright in a secluded corner, and with a most earnest expression he is busied in trying to tie a straw around his nose. Now, he plays thoughtfully with the toes of his hind foot, or snatches after the wagging end of his long tail. At another time he lies on his back and has a whole heap of hay or dry leaves hugged against his belly and he tries to tie down this loose mass by drawing his tail tightly over it with his forepaws."

The Impulse to Follow: After learning to walk the raccoons would all follow me, or anyone else, with the utmost eagerness. If I ran they struggled through the grass at their best rate, giving the instinctive cry more and more shrilly as I got further away from them, and ceasing to give it when they overtook me. In the middle of the seventh month this instinct to follow began to wane. When released from their place of confinement each one tended to go on an exploring tour of his own and to make for a nearby tree. At this time they would still follow if called. A month later no one of the four would follow at all, and their period of infancy was past.

A clear case of the inhibition of one instinct by another was observed in connection with the impulse to follow. The sixth raccoon which came into my possession had been made cross by teasing and would snap at any one who attempted to touch her. Notwithstanding this when I set her free in the meadow she followed me as the others had done. When I stopped she overtook me and halted at my feet. I then stroked her and picked her up. This I repeated many times. There was no snapping during this exercise in following, but when I tied the raccoon in her usual place and tried to pick her up she snapped at me viciously. Thus the following instinct, which is characteristic of the young raccoon, inhibited, momentarily, the fighting instinct which had been developed early in this animal by teasing. With the four full grown raccoons the condition is reversed. They will not snap at one who tries to pick them up when in their room, but they may do so when they are free.

Fear: As I have already stated, the raccoons showed fear. Like human beings they start at sounds but not nearly so violently nor so frequently as two coyotes which I had. The latter, of course, must escape from their pursuers by flight alone while the raccoon depends upon climbing and hiding. Like the dog they come to distinguish a threatening tone of the trainer and to retreat on hearing it. If hungry this has little effect on them for the thoughtful experimenter will give his animals little cause to fear him.

The sudden darkening of the room caused by the door's blowing shut produced in our young animals a panic of fear for a moment. This, with their fear of the young raccoon which I placed among them, is the only instance of fear of things which they saw.

Anger: The natural history accounts describe the raccoon as especially good-natured and so he is. Yet anger or ferocity was observed in these animals at about the twelfth week of their age. Though scrupulous care was taken to keep the animals tame they became fierce if they were left without being handled for a few days. In the fighting attitude the ears are laid back, the head lowered and the posterior portion of the body sharply humped up. Growling and unfleshing the teeth accompany this fighting attitude and, when provoked the raccoon is an ugly fighter. Intense anger and fear both act as purgatives.

Curiosity: All observers credit the raccoon with the possession of curiosity and he is likened to the monkey in this respect. The behavior which indicates curiosity is, however, associated chiefly with the sense of touch. The animals continually employ this sense in investigating the floor and all objects about them. Especially dark places, as your pocket, a knothole or a crevice, are explored by touch hundreds of times. It is no acquired trick that the raccoon puts his paw in your pocket, but a deeply rooted instinct. Even when hungry a raccoon which knows perfectly how to get food by operating some mechanism will stop work for a time in order to thrust his paw in your pocket or deep under your coat. Without exception they all carefully explored the bottom of the basin whenever fresh water was brought to them.

It may be that in nature they secure food by exploring dark crevices by touch but as my animals never obtained food in this way we should expect the impulse to have passed away unless it were genuine curiosity of a "disinterested" type.

A part of Beckmann's account of this behavior may be quoted, "Among the conspicuous peculiarities of the raccoon must be reckoned his boundless curiosity, * * * and his propensity for rummaging every nook and corner. * * * If he can get to a wall of masonry he scratches the mortar out of the joints with his sharp claws and causes incredible devastation in a short time. Like Jeremiah at the ruins of Jerusalem, he then sits down in the midst of his heaps of rubbish, looks sadly about him, and exhausted by his hard work, lifts his collar with his forepaws."

Sympathy: (?) These animals pay almost no attention to one another. Though one raccoon is retreating from you, growling and snapping, the others are in nowise disturbed. Their indifference to each other's behavior could hardly be more marked, and this fact must be taken account of in considering the question of imitation.

Dipping Their Food in Water: This peculiar instinct gives the raccoon two names. The Germans call him the "Waschbär," while science entitles him "Procyon lotor." Yet my raccoons did not always dip their food in water. No doubt this was partly due to their being fed together so that they formed the habit of eating rapidly. Moreover, they were, at first, fed only for operating some problem-box and this may also have tended to inhibit the instinct. Nevertheless, I do not believe that the raccoon in his native state will carry food very far for the purpose of "washing it." The impulse is present, of course, but it is no more invariable than any other instinct, and it has probably been observed most often in pet raccoons with a vessel of water always near them.

Sexual Instinct: No certain evidence of the presence of this instinct was observed until the twelfth month.

HABITS

Using the word habit in the natural history sense I may mention briefly some characteristic traits of the raccoon's behavior. In drinking they lap the water or milk but with the nose very close to the surface of the liquid. When heated by exercise they pant like dogs but with the tongue only slightly protruded.

There are two rather characteristic positions in sleeping. In one the animal lies on his back with his forepaws placed over his eyes. A young raccoon, when held and somewhat frightened, also puts both forepaws over his eyes, thus giving a somewhat comical appearance, suggestive of "hiding its face in its hands." Another position in sleep consists in rolling the body almost into a ball with the top of the head placed flat on the floor between the forelegs. In this position even the ears are hardly visible. Though the animal does sleep in other positions these two are most common. It would seem that the raccoon sleeps best, therefore, with his eyes not only closed but covered, and that for protection he depends most upon his lofty nest and its concealment from enemies.

Brehm says, "In May the female brings forth two or three, according to Audubon four to six, very small young ones in a rather carefully prepared den in a high tree." There were four young animals in the litter which I secured.

VISUAL DISCRIMINATION OF SIZE AND FORM IN THE ALBINO RAT

K. S. LASHLEY

From the Psychological Laboratory of The Johns Hopkins University

One figure

In the exact experimental work of comparative psychology evidence of visual image formation has been obtained for all of the higher groups of vertebrates, but few investigators have attempted to determine the accuracy of the animal's perception of outlines.

L. J. Cole, (1907) obtained evidence of the perception of size in the frog. Casteel, (1911) working with the turtle, found that it could distinguish slight differences in the breadth and direction of lines, but did not discriminate between similar patterns. For the birds, Porter, (1904-06) has demonstrated the ability of the English sparrow and cow-bird to discriminate printed patterns and, less accurately, tridimensional objects. In his work with the dancing mouse Yerkes, (1907) obtained no reactions to form and concluded that the perception of form has little importance in the activities of the mouse. Waugh, (1910) in similar experiments, obtained only slight evidence of form discrimination and was led to the same conclusion. By L. W. Cole, (1907) the raccoon has been trained to distinguish perfectly between objects of different size and form, square and circular cards, etc. Kinnaman, (1902) found that his monkeys could compare the size of the food boxes used and form some absolute standard of comparison. They also distinguished between vessels of different form but did not recognize differences in printed patterns.

The present investigation has attempted to determine the visual acuity of the rat, making use of the "form and size methods" (Washburn, 1908). Like the mouse, the albino rat is without an area centralis (Chievitz, 1891 and Slonaker, 1897), and the retina contains only rod-like sensory elements. From the likeness of structure and habitat, a similarity of visual function is to be expected.

The author is indebted to Professor John B. Watson for helpful suggestions and criticism, and to Miss Cora D. Reeves for the data in experiment 11 and for valuable assistance in other parts of the work.

APPARATUS AND METHODS

Throughout the greater part of the experimental work a discrimination box, modified after the type designed by Yerkes, (1907), was employed. This box was used by Hoge and Stocking, (1912). A diagram of it is given in figure 1. The box con-

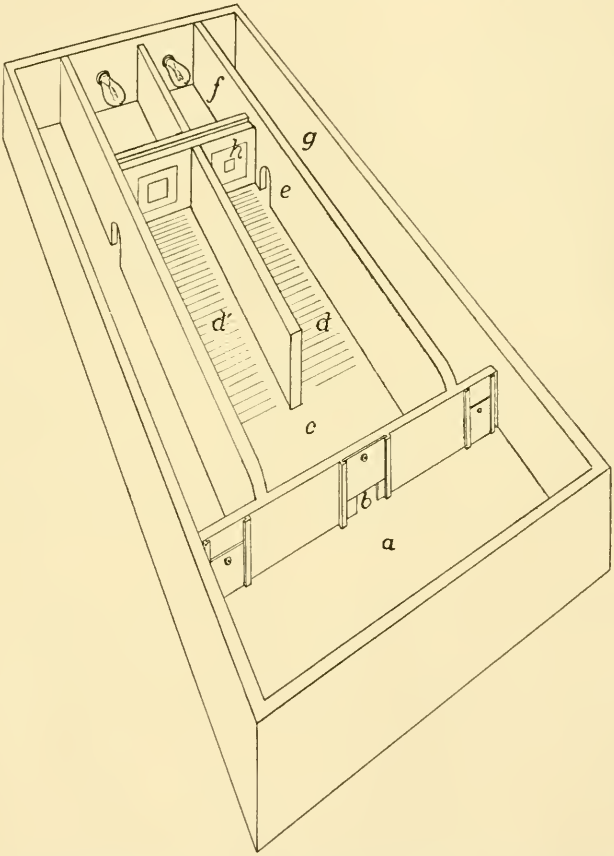

FIGURE 1. Discrimination box. a, Starting compartment; b, sliding door; c, discrimination compartment; d, d′, passages wired with electric grill; e, door to food compartment; f, light compartment; g, food compartment; h, reversible frame containing the translucent forms used as stimuli.

sists, essentially, of a starting-room, a, leading by a door into the discrimination compartment, c, in which the choice of two passages is offered to the animal. The forms to be discriminated are placed at the ends of the passages so that the animals must go directly toward them in order to reach the doors leading to the food compartments, g, g. The forms consist of metal plates, pierced by suitable openings and backed by opal flashed glass. They are fastened in a wooden frame, h, which may be lifted out of the box and inverted in order to transpose the stimuli. Sixteen candle power, carbon filament lamps, placed in a compartment, f, behind this frame, illuminate the forms brightly. The passages, d, d, are wired with an electric grill by which punishment may be administered.

Food, given in the compartments g, g, after a correct choice, and punishment, given in the passages after an incorrect one, were used as motives for discrimination. The rats were allowed to run from the starting box to the food compartment and were then lifted back to the starting box. In the first experiments punishment did not seem to give good results and it has been used as a motive only where indicated in the descriptions of experiments.

Since none of the work was quantitative, no regular system was followed in reversing the stimuli, but care was taken that the number of times that the positive stimulus was exposed in each passage should be equal in any given series of trials. In spite of this the animals, especially those which were punished, showed a strong tendency to form position associations, going again and again through one of the passages even when severely punished.

At different times during the experiment some of the rats learned to react to the noise made in reversing the forms, to the position of the experimenter during the trials, and possibly to variations in the time between successive trials. They usually tended to avoid the passage in which they had last been punished, and learned readily to follow simple rhythms of alternation. In experiments with Rat No. 6 it was customary to electrify the punishment grill before admitting the animal to the discrimination compartment, and there is some evidence that she learned to test the grill with her vibrissae before venturing over it.

Whenever any such reactions were discovered, immediate attempts were made to eliminate them, and control series were given to determine their importance in the total reaction. In general, it was only in the more difficult problems that the animals fell back upon these secondary criteria. The sense of smell never seemed to play any part in the reaction.

As a rule, each animal was given 20 trials a day, but this number could not be adhered to strictly. With the type of apparatus used, 50 per cent of error indicates that the animals are not discriminating, that their movements are due to chance, and it was found during succeeding experiments that a much lower percentage of error, not more than 25, upon several consecutive days was necessary to give conclusive proof of discrimination. An animal might make 90 per cent of correct choices in a series of 20 trials, but such a high average, if due to chance, never persisted through two consecutive series. The percentage of error is not always a safe measure of the animal's ability to discriminate between the stimuli. It does not take into account the fluctuations of attention or the difficulties of associative and learning processes. For this reason the behavior of the rats in the discrimination compartment has been given special emphasis.

ANIMALS

In the experimental work albino rats alone were used. Trapped specimens of *Mus rattus* proved to be too wild for experimental purposes, and domesticated races of pigmented rats have shown no marked superiority to the albinos in visual discrimination, so no extensive comparison between pigmented and non-pigmented races was undertaken.

The rats used differed considerably in age and in behavior at the beginning of the experiments.

No. 1. Female, 11 weeks old when given this problem; vigorous, active, and stable.

No. 2. Female, from the same litter as No. 1; very large and inactive.

No. 3. Female, from the same litter as No. 1; stunted in growth, very excitable, and easily frightened.

No. 4. Female, about two months old; large and active.

No. 5. Female, from same litter as No. 4; ill during the first part of the experiment, small but very active.

No. 6. Female, age unknown; she had given birth to one litter which she devoured; very tame, accustomed to being handled. Judged from her general behavior, this rat seems to use vision to a greater extent than the others.

No. 7. Female, age unknown; forms position associations very readily.

EXPERIMENT 1. DISCRIMINATION OF FORM

Apparatus: The discrimination box; two standard stimulus plates showing a square and a circle of equal area (56.548 sq. cm.). The experiment was conducted in a darkened room. Rats Nos. 1, 2 and 3 were used. Since they showed no preference for either form, the square was chosen arbitrarily as the positive stimulus. The motive differed for each of the animals.

No. 1. Food after a correct choice only.

No. 2. Food after every trial, punishment after an incorrect choice.

No. 3. Food after correct, punishment after incorrect choice.

During the first part of this experiment no precautions were taken to eliminate other than visual stimuli to which the rats might learn to react. The noise made in reversing the stimuli and other movements and sounds made by the experimenter were thus available to the animals as indices of the passage to be chosen.

Table I, giving the percentage of error in each 100 trials, shows the record made by the animals in 1000 trials. The percentage of error was never small enough to justify the conclusion that the animals were reacting to the difference in the visual stimuli. The occasional low percentages are to be explained by the appearance of reactions to sound, or to other accidental stimuli.

TABLE I

Discrimination Between Square and Circle of Equal Area. Percentage of Error in Each Consecutive Hundred Trials

No. trials	100	200	300	400	500	600	700	800	900	1000	Average
Rat No. 1	47	39	48	45	40	37	47	37	60	53	45.8
Rat No. 2	65	46	54	71	49	59	64	48	52	46	55.4
Rat No. 3	41	37	36	38	43	55	51	43	40	55	43.9
Average	51	40	46	51	44	50	54	43	51	51	48.3

The average percentage of error is slightly below 50 and this may be explained by the training method employed, which frequently allowed the animals to make several correct trials before the stimuli were transposed. The effect of this method appears in the first 500 trials (46.8%) but disappears in the last 500 (49.8%), in which an effort was made to eliminate all secondary criteria by which the animals might regulate their reactions.

Since the rats learned to react to sound and to other accidental stimuli it is certain that they were not indifferent to the motives provided and that their failure to respond to the visual stimuli was not due primarily to a fault in the technique of experimentation. The cause of their failure is to be sought in the nature of the visual stimuli, or in some defect in their visual apparatus. After a few hundred trials the animals became almost machine-like in their actions, the choice of the passage taken usually having a definite relation to the success or failure of the preceding trial. They rarely paused in the discrimination compartment or passages and did not seem to attend to the stimuli. It seemed, then, that the failure might be explained by this inattention and a second experiment was therefore undertaken in the hope of training the rats to attend to the illuminated areas.

EXPERIMENT 2. MOVING STIMULUS

Apparatus: The discrimination box with the forms as in experiment 1. A revolving sector was introduced into one of the light compartments (figure 1, f) between the light and the metal form. The sector was composed of two vanes, each of 45 degrees, and was turned by a small motor at a speed of five revolutions per second, thus interrupting the light 10 times per second and producing a regular flickering. It was expected that this flickering light, simulating movement, would hold the animal's attention, and that, by decreasing the size of the sector and increasing its speed, the factor of movement might be gradually eliminated and the attention transferred to the difference in form. The square and circle were retained as stimuli and the sector was kept constantly behind the square, the forms being reversed as usual.

In the first series of trials given after the introduction of the sector all the rats showed evidences of fear, refusing to go toward the interrupted light or, at first, even into the discrimination compartment. In accordance with the custom of training against a preference the rats were required to select the interrupted light. After the first 20 trials the animals gave no further signs of attention to the flickering stimulus. Increase or dimunition in speed of rotation produced no change in their behavior. After 200 trials rat No. 2 became stubborn under punishment and had to be removed from the experiment.

As shown in table II, there was no decrease in the per cent of error after 500 trials. The rats did not learn to react to the stimulus although at first they had seemed to be disturbed by it.

EXPERIMENT 3

The size of the sector and its speed of rotation were next increased so that a marked difference in the brightness of the forms was produced.* For rat No. 1 a sector of 270 degrees was used. She learned to react to the difference in brightness and a change to the original speed of rotation did not affect the reaction. The size of the sector was then reduced to 180 degrees and later to 90. Each change was followed by a sudden increase in the per cent of error, then a gradual improvement in discrimination. When the sector was reduced to 75 degrees the rat failed to discriminate and soon ceased to attend to the stimulus (table III). The chief interest of this experiment lies in the fact that by a gradual increase in the similarity of the stimuli the rat could be trained to respond to a difference to which it had formerly given no attention.

EXPERIMENT 4

Since the animals did not distinguish between the square and circle, another experiment, apparently involving a greater difference, was undertaken. A circle, 30 millimetres in diameter, was used as the positive stimulus and two circles, 21.2 millimetres in diameter and 40 millimetres apart in a horizontal line, as the negative. Rats Nos. 3 and 7 were given this problem.

Table IV gives the percentage of error for each 100 trials and shows that there was no discrimination of the stimuli, even after 1000 trials. The animals ran through the discrimination compartment without a pause and never attended to the stimuli.

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* Hoge and Stocking (1912), have shown that differences in brightness are discriminated by both albino and black and white rats.

TABLE II

Percentage of Error in Daily Tests with Interrupted Light

Rat No. 1		Rat No. 2		Rat No. 3	
Trials	%	Trials	%	Trials	%
20	40	20	45	20	50
20	35	20	40	20	65
20	45	20	60	20	35
20	35	20	65	20	45
20	40	20	35	20	45
20	30	20	30	20	80
20	55	20	35	20	80
20	30	20	40	20	40
20	35	20	40	20	55
20	30	20	50	20	55
20	35	20	40	20	55
20	45	20	55		
20	50	20	50		
20	50	20	25		
20	50	20	40		
10	50	20	55		
20	40	20	85		
20	50	20	50		
20	45	20	50		
20	45	20	35		
20	45	20	45		
20	60	10	40		
20	45	10	60		
20	45	20	55		
20	90	10	60		
20	70	10	50		
20	50	10	60		
20	80	10	50		
20	40	10	60		
		10	40		
		10	70		
630	47.4	560	59.8	220	55.0

TABLE III

Percentage of Error with Large Sector. Rat No. 1

Size of Sector	No. Trials	%
270	20	40
"	20	40
"	20	25
"	20	35
"	20	45
"	20	15
"	20	35
"	20	30
"	20	10
180	20	40
"	20	25
"	20	20
"	20	10
90	10	20
"	20	50
"	20	25
"	20	45
"	10	30
"	20	15
"	20	35
"	20	10
75	10	20
"	20	50
"	20	55
270	20	0
90	10	50
"	10	40
"	10	20
"	10	10
45	10	20
90	10	60
"	10	20
"	10	60
"	10	50
"	10	70
"	10	30
"	10	30
"	10	30
"	10	30
"	10	10
"	10	30
"	10	20
"	10	20

TABLE IV

Discrimination Between One and Two Circles of Equal Area. Percentage of Error in Each Hundred Trials

No. trials	100	200	300	400	500	600	700	800	900	1000	Average
Rat No. 7	48	50	54	58	55	59	54	61	54	56	54.9
Rat No. 3	54	47	60	56	62	48	48	47	56	51	53.1

EXPERIMENT 5

This experiment was similar to the preceding, but instead of circles the forms used were two illuminated lines, 2 by 60 millimetres, one horizontal and one vertical. Rat No. 2, at the age of seven months and after about 2000 trials in other experiments, was given this problem After the experiment with interrupted light she had been trained to react to differences in brightness so that, at the beginning of this experiment, she attended to the illuminated forms. After 150 trials she began to show evidence of discrimination, hesitating and apparently looking at the forms before choosing one of the passages.

Table V gives the daily records for 760 trials. After 300 trials there was constantly more than 80 per cent of correct choice, and since control series in which both lines were placed horizontally gave 50 per cent of error, this was taken as sufficient evidence of discrimination. Control series in which the relative brightness of the forms was altered did not affect the reaction, while any great change in the shape of the forms themselves caused a complete failure.

EXPERIMENT 6

When the reaction to the lines was firmly established an attempt was made to determine the animal's ability to distinguish lesser differences. For this purpose two of the standard forms were provided with sliding metal plates, adjustable in two directions, so that the openings could be varied from a narrow line to a square or rectangle. These were first set as lines, 20 by 60 millimetres, and substituted for the horizontal and vertical lines used in the preceding experiment. By shortening and widening the lines the contrast between them could be reduced until both became squares, equal in area.

TABLE V

Discrimination Between Vertical and Horizontal Lines. Rat No. 2

Trials	Per cent Error	Trials	Per cent Error	Trials	Per cent Error	Trials	Per cent Error
10	40	140	30	340	30	560[1]	40
20	40	150	30	360	10	580	15
30	50	160	20	380	10	600	20
40	30	180	40	400	15	620	25
60	50	200	35	420	25	640	20
80	30	220	25	440	25	660	10
90	60	240	30	460	20	680	15
100	40	260	45	480	10	700	20
110	30	280	15	500	5	720	25
120	50	300	15	520	20	740	20
130	50	320	15	540	10	760	10

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[1] At this point in the experiment No. 2 developed a bronchial disease which has made her later records somewhat irregular.

Table VI shows the changes made in the form of the stimuli and the percentages of error obtained in this experiment. Rat No. 2 gave certain evidence of discrimination between the 12 by 50 millimetre rectangles, and seemed to recognize the lesser differences without great difficulty.

TABLE VI

Discrimination of Form. Rectangles with Their Long Diameters at Right Angles

Size of Rectangles	No. Trials	Per cent Error	Size of Rectangles	No. Trials	Per cent Error
2 x 60	20	10	12 x 50	20	25
"	20	10	"	20	20
"	20	15	"	20	10
			"	20	15
12 x 50	20	25			
"	20	25	15 x 40	20	20
"	20	20	"	20	25
"	20	10	"	20	20
"	20	5			
"	20	20	20 x 30	20	10
"	10	10			
"	20	15	25 x 25	20	60
"	20	20			
			20 x 30	20	45
			"	20	60
			"	20	30

The percentage of error did not increase above 25 at any time during the experiment and the fact that no perfect records were made is due, at least in part, to the state of the animal's health. She suffered at this time from a pulmonary trouble which constantly distracted her attention from the stimuli.

Control experiments showed that the rat was not depending upon brightness, sound, or position in making the discrimination and her behavior in the discrimination compartment offers the best evidence that she was really attending to the visual stimuli. During experiments 1 and 2 she had always hesitated for a long time before venturing from the starting box to the discrimination compartment, finally dashing through to the food box as quickly as possible, avoiding the punishment grill by long leaps. When a marked difference in the brightness of the forms was introduced she began to pause in the discrimination compartment, swaying back and forth between the two passages before choosing. As her proficiency in discriminating differences in brightness increased, the time spent in choosing diminished until the reaction became almost instantaneous. With the greater number of correct choices her attempts to avoid the punishment grill were less marked. With the introduction of the lines as stimuli the hesitation between the passages again appeared, increasing and diminishing gradually after each change in the stimuli.

From the percentages of error it is evident that the animal could distinguish between the narrower rectangles. Her behavior in the discrimination compartment indicates that the discrimination was more accurate than the percentage records would seem to show, since they include all the trials made without regard to accidental distraction of attention. After the first day's work with the 20 by 30 millimetre rectangles, when 18 out of 20 trials were correct, the form of the stimuli was changed as a control. With the new forms there was 60 per cent of error, showing that perception of form was involved in the former reaction, but unfortunately this short interruption was sufficient to break up the association and in the next 300 trials there was 45 per cent of error with complete loss of attention.

EXPERIMENT 7. DISCRIMINATION OF SIZE

Apparatus: The discrimination box; two circles of 30 and 50 millimetres diameter respectively as stimuli. In the type of apparatus used the illumination of the two circular openings used as stimuli is equal per unit area, but since the flashed glass forms a total diffusing surface it acts as a direct source of illumination for the two passages. Consequently the brightness of the passages differed appreciably, being directly proportional to the areas of the circular openings. For the experimenter the difference in size between the stimuli was so much more evident than the difference in brightness that this latter was not considered at the beginning of the experiment.

Rats Nos. 4, 5 and 6 were used, with food, punishment, and food and punishment as motives. After 200 trials all began to show evidence of discrimination, but it required more than 500 trials to establish the association thoroughly. Even after 700 trials No. 5 was very uncertain in her reactions. Table VII shows the daily percentages of error in this experiment.

TABLE VII

Discrimination of Size. Circles 30 and 50 Millimetres in Diameter as Stimuli

No. of Series	Per cent of Error			No. of Series	Per cent of Error		
	Rat No. 4	Rat No. 5	Rat No. 6		Rat No. 4	Rat No. 5	Rat No. 6
1	45	25	55	23	15	25	40
2	55	60	35	24	20	30	10
3	50	40	45	25	25	25	0
4	45	40	35	26	15	30	10
5	40	45	40	27	20	35	5
6	55	45	35	28	25	20	15
7	55	40	45	29	10	30	20
8	35	70	60	30	20	20	10
9	40	40	20	31	15	35	0
10	45	45	45	32	5	15	20
11	30	35	60	33	10	35	0
12	45	45	50	34	5	10	20
13	40	65	45	35	0	25	20
14	35	25	40	36	10	15	10
15	40	45	45	37	0	25	10
16	45	35	15	38	5	40	20
17	35	35	30	39	0	15	20
18	30	65	40	40	15	20	30
19	40	10	35	41	0	0	10
20	15	55	35	42	0	30	20
21	10	35	30	43	0	30	0
22	30	55	10				

Under the conditions of the experiment it was uncertain whether the animals were reacting to the difference in size of the circles or to the difference in the brightness of the passages. A few control tests (table VIII) showed conclusively that the discrimination had been made upon the basis of the difference in the brightness of the passage only. The rats had been choosing the larger circle and when the relative brightness of the two was changed they chose always the brighter passage, regardless of the size of the forms. The slight difference in brightness was evidently more easily associated with the motive than was the difference in the size of the circles.

TABLE VIII

CONTROL ON EXPERIMENT 7

Nature of the Stimuli	Rat No. 4		Rat No. 5		Rat No. 6	
	Trials	Per cent Error	Trials	Per cent Error	Trials	Per cent Error
Illumination of larger circle reduced; brightness of passages equalized....	50	44	30	60	10	50
Size of circles equal; illumination unequal; animals chose the brighter...	10	0	10	10	..	..
30 and 50 mm. circles; the 30 mm. more brightly illuminated.........	..	..	20	95	10	50
30 and 50 mm. circles; the brightness of the passages equalized by reflected light......................	10	40	10	60	10	50
30 and 50 mm. circles; the floor of the passage to the 30 mm. illuminated brightly........................	10	60	10	80	10	60

EXPERIMENT 8

The preceding experiment was continued with the element of brightness eliminated. A 32 c. p. lamp was suspended six feet above the experiment box, so that the two passages were brightly illuminated (table IX, illumination 1). Later, the intensity of this light was increased until the difference in the brightness of the passages was reduced below the human threshold and, consequently, much below that found to exist for the rat (table IX, illumination 4). The 30 and 50 millimetre circles were retained as stimuli.

At first the animals failed to discriminate between the circles, but after 100 trials No. 4 showed evidences of discrimination, and in 200 trials learned to choose the larger circle accurately.

The other rats were slower in learning and required from 600 to 800 trials. When they had learned the problem they were given control series similar to those outlined in table VIII, but in this case the results were quite different. Changes in the relative brightness of the passages and forms did not affect the accuracy of the reaction, while reduction of the difference in size caused an increase in the per cent of error. In order to make certain that the rats were not reacting to the movements of the experimenter a test series was given by another person and although the animals were excited by the change they still made above 70 per cent of correct choices.

TABLE IX

Discrimination of Size. Brightness Factor Eliminated

Rat No.						Rat No.						Rat No.					
4		5		6		4		5		6		4		5		6	
No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error	No. Trials	% Error
10	50	10	60	10	40	20	0	10	60	20	45			20	30	20	15
10	30	10	50	10	50			10	30	20	45			20	25	20	20
10	90	10	30	10	50			10	40	20	50			20	25	20	15
10	30	10	50	10	50			10	40	10	30			20	20	20	5
10	20	10	50	10	50			20	50	10	50			20	20	20	10
10	30	10	10	10	50			20	50	10	30			20	10		
10	15	10	50	10	20	Illumination 1								20	10		
10	30	10	20	10	30			10	30	10	50			Ill. 4			
10	20	10	30	10	30			20	75	20	25			20	30		
10	15	10	20	10	30			20	55	20	25			20	20		
20	35	10	50	10	40			20	25	20	10			10	30		
20	10	10	40	10	40			20	60	20	55			10	20		
20	5	10	50	10	40			20	50	20	20			10	30		
20	10	10	40	10	30			20	35	20	15			10	20		
20	10	10	30	10	60			20	45	20	25			10	10		
20	5	10	40	10	70			20	20	20	10			10	90		
Illumination 4								20	20	20	10			10	50		
20	20	10	50	10	50			20	35	20	25			10	20		
20	10	10	20	20	50			20	15	20	10			10	30		
10	10	10	60	20	45			20	15	Ill. 4							
												300.		790.		630.	

EXPERIMENT 9

As soon as definite evidence of size discrimination was obtained an attempt was made to determine how small a difference the animals could distinguish. The size of the smaller circle was increased to 40 millimetres, the 50 millimetre circle being retained as the positive stimulus. Rat No. 4 learned to distinguish between these circles, but after 200 trials grew inattentive. Punishment was introduced to strengthen the motive and was continued throughout the third 100 trials (table X). The punishment was not associated with the stimulus and served only to confuse the rat so its use was abandoned. After 200 more trials the association was restored, but further attempts to reduce the difference between the stimuli produced an uncertain reaction. The smallest difference which the rats can distinguish, under the conditions of the experiment, is about two-fifths of the area of the larger circle.

TABLE X

Discrimination Between Circles of Different Size. Rat No. 4

Size of Circles	Trials	Per cent Error
50 and 30	10	0
50 and 40	20	15
" " "	20	20
" " "	20	10
" " "	20	25
" " "	20	0
" " "	20	20
" " "	20	10
" " "	20	15
" " "	20	25
" " "	20	40
" " "	20	25
" " "	20	35
" " "	20	30
" " "	20	50
" " "	20	60
60 and 30	20	5
50 and 30	20	40
50 and 30	20	15
" " "	20	40
" " "	20	20
" " "	20	20
" " "	20	20
" " "	20	20
" " "	20	20
" " "	10	10
" " "	20	10
50 and 40	10	30
" " "	10	30
" " "	10	30
" " "	10	20
" " "	10	10
50 and 45	20	60

EXPERIMENT 10

In the preceding experiments it appeared that the principal fault in the method of experimentation lay in the fact that the rats did not attend readily to the stimuli. Those which learned to react to differences in size or form had first learned to attend to brightness differences. From this it seemed probable that animals which were attending to the stimuli and reacting to differences in size might be trained without great difficulty to attend to differences in form.

Two forms, a square of 1600 square millimetres area and a circle of 706 square millimetres, were substituted for the forms used with rat No. 6 in experiment 9. The animal (No. 6) discriminated between these immediately, choosing the square. The area of the circle was gradually increased to 1200 square millimetres at which point the rat failed to react. This process of starting with a wide difference in area and gradually decreasing it was repeated three times, but whenever the area of the circle became more than three-fourths that of the square the rat ceased to react to the difference. After 600 trials there was no evidence that she could discriminate form.

With rat No. 4 a six pointed star was used as the positive stimulus, a circle of varying size as the negative. No. 4 chose the star so long as the difference in size was great, but when the area of the circle became more than three-fourths that of the star, the reaction became uncertain or broke down entirely.

With rat No. 5 two circles of a combined area of 706 square millimetres area (see experiment 4) were used as the negative stimulus, and a square of varying area as the positive. Starting with a square of 3600 square millimetres area, the area was reduced to 1225, at which point discrimination failed. This was repeated twice but the reaction always became uncertain when the difference in size became less than the area of the smaller form.

Later, with rat No. 6, a 40 millimetre square was used as the positive stimulus, and the negative stimulus was varied in form, its size being kept less than three-fourths that of the square. Circles, triangles, and various other forms were used. After the first few changes the rat was no longer disturbed by alterations in the form of the stimulus and chose the larger without hesitation. But whenever the difference in size was reduced below the threshold (experiment 9) she fell back upon position associations and failed to discriminate. In several of these series the rat chose the form of greater area, even when the other appeared larger when viewed as a unit of light and shade.

From these experiments it appears that the rats, in discriminating between objects of different size, do not perceive or do not attend to differences in form. Further data, obtained from rats 4 and 6 when offered a choice of two new forms of unequal size, support this view.

EXPERIMENT 11

Since the discrimination box places the animals under somewhat unnatural conditions a few tests were undertaken under an environment more nearly approaching the normal.

(a) Two cardboard food boxes were constructed, the one rectangular, the other pyramidal in form and having small openings in the sides to admit the rats. The exposed faces of the boxes were of equal area (128 square centimetres) but, owing to the sloping surface of the pyramid, they differed somewhat in brightness. Food was given in only one of the boxes and they were interchanged irregularly to avoid position associations.

Of the three rats required to choose between the boxes not one showed any evidence of discrimination after 150 trials. All formed position and rhythm associations but did not distinguish between the boxes in any way.

(b) Rat No. 1 was trained to go to a large square of flashed glass supported in an upright position upon the floor of the laboratory. After a few trials she learned to run directly to the square from a distance of two or more feet, passing behind the glass to get a bit of food. When this association was established a small piece of glass, one centimetre square, was placed beside the larger (12 by 12 centimetres). The rat sniffed at the small glass only once, then gave it no further attention. When a plate of 25 square centimetres area was substituted for the smaller square, the rat became confused and failed to choose the larger. After 40 trials, however, she began to avoid the small square and made a record of 17 correct choices in 25 trials. A rectangle of half the area of the larger square was then used as the negative stimulus. In the first 20 trials the rat made only five per cent error, then became frightened and in 200 trials since has shown no evidence of discrimination.

From this experiment it appears that the rat can distinguish great differences in size without difficulty. With slighter differences, however, the dependence upon kinaesthetic sensations appears, just as in the discrimination box, and learning does not seem to be any more rapid. The objection to the unnatural-ness of the discrimination apparatus does not seem, then, to be a very serious one.

EVIDENCE FROM THE RAT'S BEHAVIOR IN THE DISCRIMINATION COMPARTMENT

In the descriptions of the experimental work the percentage of error has been employed to express the rat's ability to discriminate, since a great number of correct choices forms the most certain evidence, but in all the experiments, where positive results were obtained at all, the rats showed a recognition of the difference between the forms before the percentage of error was reduced below 50. In the unsuccessful experiments, after the first 100 trials, the animals became almost machine like in the regularity of their movements. There was seldom a pause in the discrimination compartment and the choice of a passage usually followed some definite rhythm. In experiments 3, 5 and 7 the beginning of discrimination was marked by long hesitation with a swaying back and forth between the passages. In the early stages of learning a correct choice more frequently followed where the animal hesitated than when it made a rapid choice. Later in the experiment choice by negation seemed to become the more important method of eliminating error. The rats chose a passage quickly, and if it were wrong, turned back when about eight inches from the stimulus. Choice by affirmation never seemed to be very effective, even with the best trained rats. They would frequently make a correct choice, then hesitate, turn back, and compare the forms.

When responding to visual stimuli they showed a careful attention to the illuminated forms, frequently returning from the food compartment to sniff at them after an incorrect choice. Failures more often followed after evident inattention.

At the introduction of any new element into the discrimination box, as when swinging doors were placed in the food boxes or when the passages were repainted, the rats became very much excited, refusing to leave the starting box or to approach the new object. This type of behavior was very marked in experiment 2 when the revolving sector was introduced, and shows unmistakably that the rats perceived the moving shadow, although they did not associate it with the motive. Changes in the brightness relations of different parts of the box were followed similarly by confused reactions.

The responses to sudden changes in the form of the stimuli are of importance as evidence of discriminating ability. Rat No. 6 was choosing the larger of two circles accurately (100%). Two squares of the same area, respectively, as the circles, were substituted for the circles and during the next 10 trials No. 6 was greatly disturbed, running in and out of the starting box, creeping a little way down the passages, then turning back. She attended to the squares as to something new. There had been no noise to account for this behavior and it is not probable that the new forms had any peculiar odor, so it seems that the rat must have been disturbed by the change of form.

This is only one instance of a type of behavior which appeared, to a greater or less extent, throughout all the experiments whenever the form of the stimuli was changed. It suggests that the animals perceived the difference, although they never associated it with the motive, and raises the question whether the discrimination method offers a fair test of the animal's discriminative ability.

SUMMARY OF RESULTS

In all the records there is a constant deviation in the percentage of error, and a series of 20 successive correct trials is very rare. Sudden increases in the number of mistakes may frequently be explained by the physical condition of the animals. If they are fed too much or too little, their attention to the stimuli is affected and the percentage of error increases. When punishment is used, the rat's fear of the punishment grill after too severe a shock causes irregularities in the records.

Making allowance for such deviations, the selection of the vertical line 164 times in 200 trials (experiment 5) proves that rat No. 5 could distinguish between the vertical and horizontal lines. In discrimination of size, the record of only 14 mistakes in 150 trials shows that rat No. 4 could discriminate between the 30 and 50 millimetre circles.

The number of animals used in the experiments has been small, necessarily, since the time required for each series is considerable. The rats show marked individual differences and all vary from day to day in their attention to the problem. The quantitative results are therefore only approximate. The tests for the threshold of form discrimination have been carried scarcely far enough to give definite results, and the data at hand are confused by the illness of the rat (No. 6) but it seems probable that she could distinguish between the 20 by 30 millimetre rectangles used in experiment 6. The difference between these forms is scarcely greater than that of the stimuli used in experiments 1 and 4, so this difference must lie somewhere near the threshold for the conditions given.

In the discrimination of size the records of experiments 9 and 10 indicate that the rat can distinguish a difference of one-fourth the area of the larger circle (2000 square millimetres). Reduction below this limit caused uncertainty of reaction and loss of attention. In addition to their ability to compare and choose between the stimuli, the rats are able to fix upon a rough standard of size. In choice by affirmation and negation they tended to go toward a circle of more than 40 millimetres diameter and to avoid one of less than 30 millimetres. But when both circles were made larger than this standard the rats still chose the larger of the two by comparison.

The long failure to react to the interrupted light in experiment 2 suggests that the factor of movement may play a less important part in the activities of the animals than is generally supposed; that an accustomed moving object is no more effective in catching and holding the attention than is a stationary one.

THE NATURE OF THE RAT'S IMAGE PERCEPTION

The rat perceives brightness differences readily, but ordinarily without attention to the nature of the image. Animals which have been trained to go to a white food dish will run to any white object. Absolute size is not so easily recognized, but greatly different areas are distinguished without difficulty. The perception and recognition of form seem to be most difficult for the animal and to require an unusually close attention. In these respects the rat's visual perceptions resemble closely human perception in the extreme peripheral field. Brightness is most evident and size is more readily perceived than form.

TRAINING METHODS

The work of Hoge and Stocking, (1912) indicates that punishment is more effective than reward as a motive for visual discrimination. In the present experiments it was found that, with the more difficult problems, punishment as a motive had serious disadvantages. With the dancing mouse Yerkes finds punishment to be more efficient than hunger. But unlike the dancing mouse, the adult albino rat is inactive and so a strong motive must be used to induce it to go through the discrimination box. Food furnishes practically the only available motive and must be used almost constantly if the time of reaction is to be kept within convenient limits.

When punishment is introduced in connection with food there is an immediate slowing of the reaction. As a general rule this is evidenced by a hesitation in leaving the starting compartment rather than in choosing one of the passages. The animals show a tendency to go through the discrimination compartment and passages in as few leaps as possible, frequently determining their direction before leaving the starting box.

The difficulty in regulating the severity of the punishment forms the chief objection to its use. The rats differ greatly in sensitivity to the current; No. 3 showed evidence of pain with a shock which the experimenter could not detect, while No. 2 gave no reaction to a current many times as strong. The same rat also differs in sensitivity at different times, and a single instance of too severe punishment is apt to lead to the formation of position habits which can be broken up only with the greatest difficulty. In a difficult problem, with long continued failure to discriminate, the animals become stubborn under punishment and may refuse to leave the starting compartment. For these reasons the use of punishment was almost entirely abandoned. In one case only, that of rat No. 6, was it retained in all the experiments. Her record is not greatly superior to that of the others, although in many ways she appears to be the most intelligent of the group.

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THE INTELLIGENCE OF EARTHWORMS

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Five figures

"But some degree of intelligence appears to be exhibited in this work [the plugging of the mouths of their burrows with various objects by earthworms],—a result which has surprised me more than anything else in regard to worms."[1] Thus wrote Charles Darwin as a result of his careful study of the habits of earthworms. Certain of his observations and the conclusion to which he felt himself forced have been discredited by the recent work of Miss Hanel.[2] Darwin, doubtless, was overliberal in his ascription of "mental qualities" to the worm, but Hanel certainly has proved herself extremely critical and parsimonious. In spite of the honest efforts of these excellent observers, and of many other biologists who have paid special attention to the behavior of earthworms, it must be admitted that we to-day know little concerning the possibilities of habit formation in these organisms.

The present investigation, taking its start from the work of Darwin, as critically repeated by Hanel, was planned for the purpose of (a) demonstrating the ability or inability of the earthworm to acquire direction-habits; (b) exhibiting the characteristics of such habits as might appear; (c) discovering external and internal factors important in connection with such habits; (d) determining the degree of permanency of habits; and (e) discovering the relation to the "brain" of such habits as appeared.

This paper is an introductory and preliminary account of an investigation which is still in progress. It is limited to a description of the general method employed and to a detailed account of the behavior of a single worm, No. 2, (*Allolobophora foetida*), which was under observation from October, 1911, until September, 1912. It is the writer's purpose to presen later a monographic account of his experiments with species of *Allolobophora* and *Lumbricus.*

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[1] Darwin, Charles. Formation of Vegetable Mould Through the Action of Worms. New York: D. Appleton and Company, 1898, p. 35.

[2] Hanel, Elise. Ein Beitrag zur "Psychologie" der Regenwürmer. Zeit. für allg. Physiol., 1904. Bd. 4, S. 244–58.

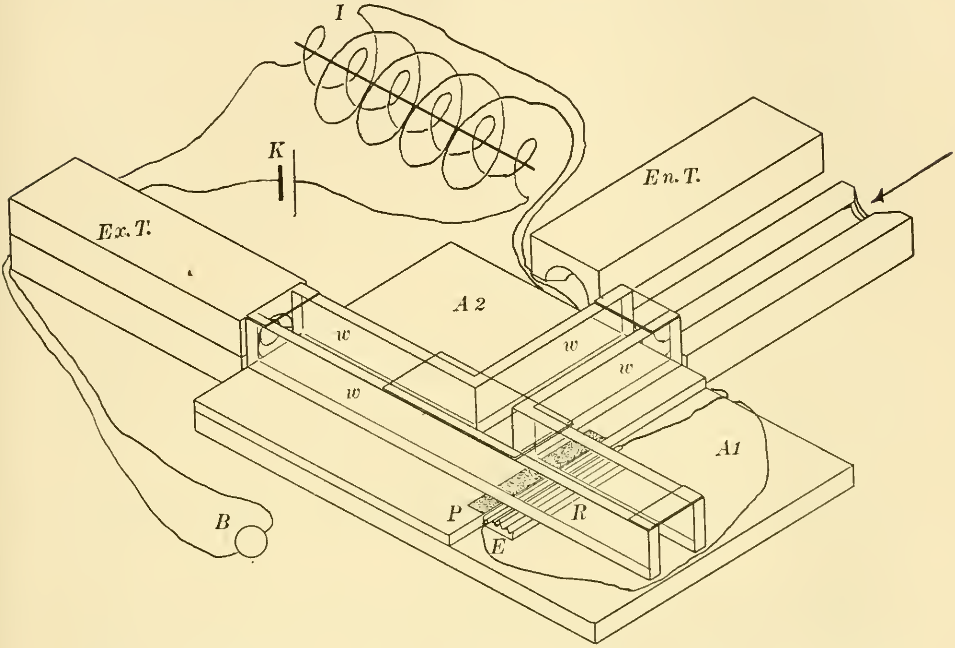

FIGURE 1. Perspective of T apparatus for study of habit formation in the earthworm. A 1, plate-glass base for parts of apparatus; A 2, layers of white blotting paper covering approximately two-thirds of A 1; w, w, w, w, plate-glass walls of T-shaped passage-way; En.T., wooden entrance tube, lined with moistened white blotting paper, from which worm enters passage-way, as indicated by arrow (the cover of the tube is shown removed); Ex.T., wooden exit tube in position for reception of worm as it emerges from open arm of glass T (in this case, the cover is in position); P, strip of sandpaper resting on A 2 and extending across passage-way; E, pieces of copper wire serving as electrodes, insulated and kept at fixed distance from one another by the corrugations of the strip of rubber, R; I, inductorium, wires from the secondary coil of which terminate in the electrodes at E; K, key in primary circuit of inductorium; B, dry cell.

In figure 1 is reproduced a perspective drawing of the apparatus used in the investigation. It consists of a T shaped labyrinth of plate glass, with runways 2 centimeters wide. The glass walls, w, are 2½ centimeters high and the distance from either entrance to the middle point of the unbroken wall is 12 centimeters. The walls are held together by four pieces of glass which are cemented to their upper surfaces with "cementium." The glass T rests upon a piece of plate glass two-thirds of which is covered, as is indicated in the figure, by layers of white blotting paper to the thickness of 5 millimeters The remainder of the base-plate is left uncovered in order that a piece of rubber, R, may be inserted under the T as insulation for two copper electrodes, E.

Wooden tubes which serve as artificial burrows appear at En.T. and Ex.T. The former may be designated the entrance tube and the latter the exit tube. The "tubes" are made by boring a 13 millimeter hole the length of a block of soft wood 14 by 4 by 4 centimeters. The block is then split. The ends of the hole are bevelled and sandpapered and the lower half is lined with white blotting paper.

A strip, 1 centimeter wide, of No. ½ sandpaper extends across one arm of the T, as shown at P, (figure 1); and just beyond the outer edge of the sandpaper, resting on the plate glass base, is the piece of corrugated rubber, R, in two of the grooves of which lie the copper electrodes, E.

In the early experiments with worm No. 2, a piece of glass carrying a strip of blotting paper which had been soaked in strong salt solution,[3] took the place of the rubber and the electrodes.

This apparatus was designed to test the ability of earthworms to "learn" to follow a simple path and to avoid an injurious chemical (or electrical) stimulus by reacting negatively to a peculiar tactual stimulus which regularly preceded the chemical. The experimenter had in mind two questions: First, can the worm profit by experience; second, can it "associate" the tactual stimulus with the chemical and acquire the habit of regularly responding to the sandpaper as tho it anticipated the effect of the salt.

In September, 1911, a number of manure worms were collected at Cambridge, Massachusetts, for this investigation. Those selected for use were kept in earthenware jars, 10 centimeters deep by 7.5 centimeters in diameter, in a moist mixture of earth and horse manure. Number 2, the subject of this report, was, from the first, an active, vigorous individual, which reacted well in the apparatus and evidently throve on the treatment afforded it. Between September, 1911, and September, 1912, it doubled its size.

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[3] The solution was made by dissolving 60 grams of c. p. sodium chloride in one liter of distilled water.

Each day, in preparation for the experiments with No. 2, the blotting paper upon which the glass T rested was soaked with tap water, as were also the paper linings of the entrance and exit tubes. The apparatus was then set with the stem of the T directed toward and perpendicular to a north window which was the sole source of illumination during experimentation. The strip of sandpaper now was properly placed beneath one arm of the T at 1 centimeter from the adjacent wall of the stem, and just beyond it, on the plate glass base, instead of on the blotting paper, a thin piece of glass supporting a strip of blotting paper soaked in salt solution was adjusted.[4]

When the apparatus was in readiness the experimenter removed the worm from its jar by inverting the latter over a dish and emptying the entire contents. Then with a fine camel's hair brush the worm was removed to a small dish of water, to wash off any adhering particles of earth. Thence it was carefully, but quickly, transferred to the lower half of the entrance tube and immediately covered with the upper half to protect it from the light.

A "trial" or "test" was given as follows: Having placed the entrance tube containing the worm at the base of the stem of the T, with the anterior end of the worm directed toward the T, the experimenter removed the cover of the tube, thus permitting the light from the window to drive the negatively phototactic worm into the T. If the light did not within a few seconds cause the subject to move forward, it was lightly stroked along the dorsum of the terminal segments with a moist camel's hair brush.

No. 2 usually started quickly, and especially in the later experiments, without being "touched."

The experimenter noted, by referring to a watch, the moment of uncovering the tube, and registered the beginning of the trip thru the T by starting a stop-watch the instant the head of the worm entered the stem of the T. He then observed minutely the behavior of the worm in the apparatus, recording as accurately as might be the path followed, the number of times the worm came into contact with the strip of sandpaper and the number of times it received the chemical (or electrical) stimulus.

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[4] In the later experiments with worm No. 2 the strip of rubber and the electrodes took the place of the apparatus for chemical stimulation.

The subject was permitted neither to retrace its course thru the stem of the T to the entrance tube, nor to escape by the arm containing the sandpaper.

The instant the "head" of the worm entered the exit tube the experimenter stopped the stop-watch and recorded the results of the trial.

For convenience, as well as accuracy, in the recording of data an outline of the apparatus, similar to that of figure 2, was cut in rubber and on the blank impressions produced with this stamp the observer recorded (a) the number of the "trial," (b) the hour, (c) the total time for the "trip," (d) the number of contacts with the sandpaper, (e) the number of chemical (or electrical) stimulations, and (f) the path followed. Supplementary notes concerning features of the behavior frequently were appended.

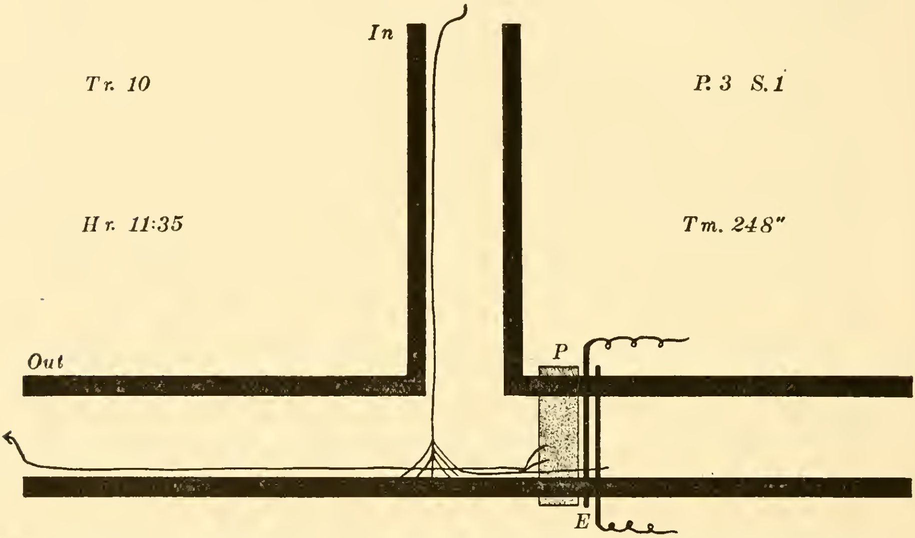

FIGURE 2. Ground plan of T, showing path followed by earthworm No. 2 in trial 10. (In this figure and the following one, fig. 3, a piece of blotting paper soaked in salt solution took the place of the electrodes at E.)

Experimentation with worm No. 2 was begun October 12, 1911. On that day it was given ten trials in the apparatus. An interval of about twenty minutes, gradually diminished to five in the later series, separated the trials.

For several weeks worms No. 2 and No. 1 were given trials alternately in order that they might obscure one another's mucous trails. From the first the sandpaper proved to be slightly repellent to No. 2. Consequently the chemical stimulus was received in only two of the first series of 10 trials. In accordance with chance, the worm turned as often to the right arm of the T as to the left arm.

Four days after the first series, on October 16, a second series yielded results strikingly similar to those of the first day, and as strikingly in contrast with those obtained on the third day.

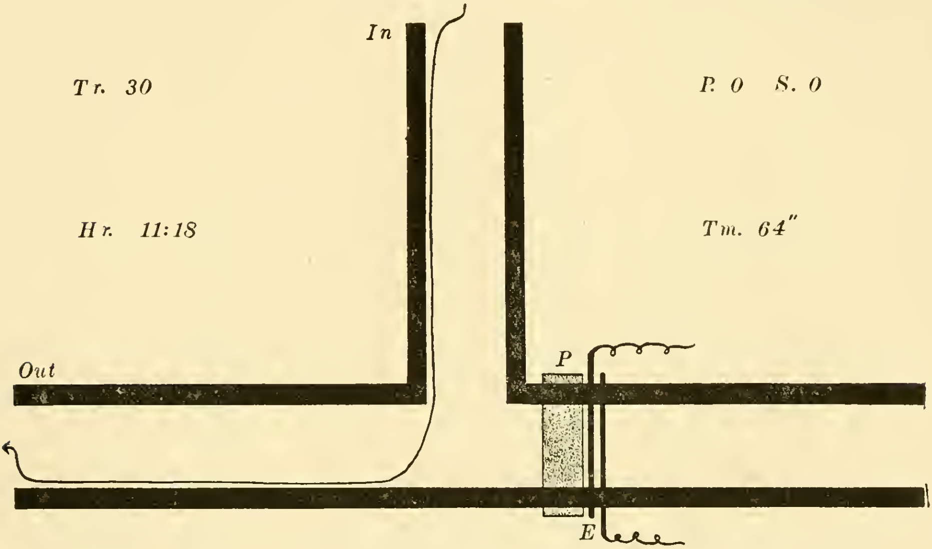

FIGURE 3. Diagram of T, showing path followed by earthworm No. 2 in trial 30.

The data for the first three series of trials are presented in table 1. In these results it is remarkable that, after only 20 experiences in the apparatus, No. 2 should have succeeded, on October 17, in passing thru the T from entrance tube to exit tube 10 times in succession without a mistake in the direction of turning and usually with directness.

In trial 21, the first on October 17, the worm followed the left wall of the stem of the T as far as the junction of the arms, then turned sharply to the right and passed directly into the exit tube. In trials 22 to 30 it, instead, followed the right wall of the stem, thus taking the shortest route to the exit tube (figure 3).

The route shown in figure 2 is typical of the early indirect journeys; that of figure 3, of the later direct trips.

As appears in table 1, the average time for the first 10 trips was 163″; for the second 10, 103.5″; for the third 10, 68.5″.

TABLE 1

Results of First Thirty Training Experiments with Worm No. 2

		Contacts with		
Date	Trial	Sandpaper	Salt	Time
Oct. 12	1	0	0	61″
“	2	2	0	84″
“	3	0	0	68″
“	4	0	0	85″
“	5	3	0	146″
“	6	2	0	173″
“	7	12	5	480″
“	8	0	0	133″
“	9	0	0	152″
“	10	3	1	248″
Average of ten trials		**2.2**	**.6**	**163″**
Oct. 16	11	1	1	126″
“	12	1	0	66″
“	13	1	0	71″
“	14	0	0	60″
“	15	3	1	159″
“	16	0	0	87″
“	17	2	1	180″
“	18	0	0	63″
“	19	0	0	114″
“	20	2	0	109″
Average of ten trials		**1**	**.3**	**103.5″**
Oct. 17	21	0	0	75″
“	22	0	0	109″
“	23	0	0	58″
“	24	0	0	46″
“	25	0	0	63″
“	26	0	0	61″
“	27	0	0	70″
“	28	0	0	56″
“	29	0	0	83″
“	30	0	0	64″
Average of ten trials		**0**	**0**	**68.5″**

These results strongly suggest to the student of animal behavior "tracking" or the directive influence of light. To the experimenter it seemed extremely improbable that the worm had acquired a perfect direction-habit as a result of only 20 trials in the T. For the present it must suffice to state that no satisfactory explanation for the correct series was revealed by check experiments. The worm apparently profited by experience with surprising quickness.

Between October 12 and November 6, as may be noted in the accompanying summary of experiments with No. 2, the worm was given 200 trials in series of 10 each. The results were variable. Thus we find that the perfect record for October 17 was followed by a record of contact with the sandpaper in three trials and with the salt in one trial on October 18, and that, in turn, by a perfect record on October 19.

Summary of Experiments with Worm No. 2

Trials	Dates	Direction of Turning	No. of Trials	Stimuli	Condition of Worm
1- 200	Oct. 12-Nov. 6, 1911	Right	10	Paper and salt	Normal
201- 400	Nov. 7-Nov. 17, 1911	"	20	" "	"
401- 510	Nov. 20-Nov. 30, 1911	"	10	" "	"
511- 645	Dec. 2, 1911-Jan. 25, 1912	"	5, 10, 15	" "	"
646- 710	Jan. 26-Feb. 13, 1912	"	5	" and elect.	"
711- 780	Feb. 15-Apr. 9, 1912	Left	5	" "	"
781- 860	Apr. 10-Apr. 30, 1912	Right	5, 10	" "	"
861- 920	May 2-June 7, 1912	"	5	" "	"Brainless"
921-1000	July 4-July 25, 1912	"	5	" "	"New Brain"

The training on the basis of 10 trials per day (with omission of experimentation occasionally) failed to establish a definitely predictable mode of response (perfectly dependable habit) in No. 2, so the number of trials per series was increased to 20, beginning on November 7. Thus the experimentation was continued until November 17, when No. 2 was given its four-hundredth trial. The results varied extremely from day to day. There appeared to be "good" and "bad" days. The data of tables 2 and 3 are offered in support of this statement.

Thus, November 8 (table 2) proved to be a "bad" day as compared with November 9. Likewise, November 10 proved to be unfavorable, whereas November 11 was favorable (table 3).

TABLE 2

RESULTS, WITH WORM NO. 2, INDICATIVE OF IRREGULARITY OF PERFORMANCE FROM DAY TO DAY. ("GOOD" AND "BAD" DAYS)

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
Nov. 8	221	0	0	110″	Nov. 9	241	4	3	204″
“	222	0	0	55″	“	242	0	0	60″
“	223	1	0	73″	“	243	0	0	59″
“	224	0	0	79″	“	244	0	0	65″
“	225	3	1	94″	“	245	2	0	104″
“	226	0	0	113″	“	246	0	0	81″
“	227	1	0	82″	“	247	0	0	68″
“	228	3	1	117″	“	248	0	0	65″
“	229	1	0	89″	“	249	0	0	51″
“	230	0	0	73″	“	250	0	0	59″
“	231	1	1	120″	“	251	0	0	62″
“	232	1	0	92″	“	252	0	0	65″
“	233	0	0	130″	“	253	0	0	58″
“	234	1	1	111″	“	254	0	0	80″
“	235	5	2	185″	“	255	2	0	81″
“	236	0	0	98″	“	256	2	0	122″
“	237	0	0	99″	“	257	2	0	100″
“	238	0	0	90″	“	258	0	0	64″
“	239	4	1	136″	“	259	0	0	157″
“	240	0	0	120″	“	260	1	0	73″
Av. of twenty trials		**1.05**	**.35**	**103.3″**	Av. of twenty trials		**.65**	**.15**	**83.9″**

TABLE 3

RESULTS, WITH WORM NO. 2, INDICATIVE OF IRREGULARITY OF PERFORMANCE FROM DAY TO DAY. ("GOOD" AND "BAD" DAYS)

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
Nov. 10	261	5	0	162″	Nov. 11	281	5	0	154″
“	262	0	0	133″	“	282	0	0	72″
“	263	0	0	97″	“	283	0	0	69″
“	264	0	0	86″	“	284	0	0	82″
“	265	0	0	81″	“	285	1	0	90″
“	266	0	0	105″	“	286	1	0	107″
“	267	0	0	87″	“	287	1	0	59″
“	268	3	1	138″	“	288	0	0	50″
“	269	0	0	100″	“	289	0	0	46″
“	270	0	0	105″	“	290	0	0	47″
“	271	2	1	119″	“	291	0	0	54″
“	272	3	2	138″	“	292	2	0	109″
“	273	7	3	239″	“	293	0	0	61″
“	274	0	0	98″	“	294	0	0	49″
“	275	0	0	154″	“	295	0	0	50″
“	276	2	0	77″	“	296	0	0	48″
“	277	0	0	73″	“	297	0	0	41″
“	278	3	1	110″	“	298	0	0	45″
“	279	2	0	107″	“	299	0	0	61″
“	280	0	0	240″	“	300	0	0	59″
Av. of twenty trials		**1.35**	**.4**	**122.4″**	Av. of twenty trials		**.5**	**0**	**67.6″**

All things considered, the results of the training by 20 trials per day were less satisfactory than those of the earlier training by 10 trials. The worm frequently exhibited fatigue before the completion of a 20-trial series. Concluding that he had been overworking his subject, the experimenter reduced the number of trials to 10 per day from November 20 to November 30,—trials 401–510.

TABLE 4

Results of First Trials in Daily Series Compared with Averages for the First Half of the Trials of the Daily Series

Trial	Sandpaper	Salt	Time	Trials	Sandpaper	Salt	Time
1	0	0	61″	1– 5	1.0	0	88.8″
11	1	1	126″	11– 15	1.2	.4	96.4″
21	0	0	75″	21– 25	0	0	70.2″
31	0	0	65″	31– 35	2.2	1.4	142.8″
41	0	0	82″	41– 45	0	0	64.8″
51	6	5	125″	51– 55	2.2	1.4	130.4″
61	1	0	120″	61– 65	5.0	2.8	165.8″
71	1	0	150″	71– 75	.8	0	122.0″
81	2	1	124″	81– 85	3.8	1.4	197.0″
91	0	0	84″	91– 95	.4	.2	104.6″
101	3	2	228″	101–105	1.2	.8	158.0″
111	3	2	167″	111–115	1.6	1.0	121.8″
121	7	6	283″	121–125	1.6	1.2	123.8″
131	1	1	149″	131–135	1.6	.8	119.0″
141	15	11	345″	141–145	3.2	2.4	137.2″
151	1	1	150″	151–155	1.4	.4	123.2″
161	14	10	345″	161–165	3.2	2.0	131.6″
171	0	0	103″	171–175	2.0	.4	113.6″
181	0	0	92″	181–185	.4	.2	84.0″
191	4	1	158″	191–195	2.0	.6	112.2″
201	3	0	89″	201–210	1.2	.3	88.2″
221	0	0	110″	221–230	.9	.2	88.5″
241	4	3	204″	241–250	.6	.3	81.6″
261	5	0	162″	261–270	.8	.1	109.4″
281	5	0	154″	281–290	.8	0	77.6″
301	7	2	222″	301–310	1.2	.4	95.3″
321	1	0	102″	321–330	1.4	.3	102.2″
341	0	0	62″	341–350	.7	.5	100.1″
361	8	0	169″	361–370	2.2	.1	98.3″
381	3	1	187″	381–390	1.5	.5	109.8″
401	4	2	289″	401–405	1.8	1.0	156.2″
411	0	0	58″	411–415	0	0	72.0″
421	1	0	113″	421–425	.6	.2	134.0″
431	4	2	279″	431–435	.8	.4	112.8″
441	0	0	104″	441–445	1.0	.4	101.0″
451	3	1	213″	451–455	.8	.2	110.8″
461	0	0	206″	461–465	.6	.2	154.2″
471	0	0	151″	471–475	.6	.6	172.4″
481	0	0	77″	481–485	0	0	73.2″
491	3	1	67″	491–495	1.2	.4	114.4″
Averages	**2.75**	**1.32**	**151.2″**	Averages	**1.34**	**.59**	**114.0″**

Table 4 presents, on the left, the results of the first trial in each daily series up to the five-hundredth trial, and, on the right, the averages of the data for the first half (5 or 10) of the trials of each daily series. It appears that more than twice as many contacts with sandpaper and salt were made in "first" trials as in the "first half" of the series. There is evidence of daily improvement in this comparison, but no proof of the acquisition of a habit which lasts from day to day.

The experimental procedure was further modified in the interval between December 2, 1911, and January 25, 1912,—trials 511 to 645—by the use of as few as five trials in a series, and by the frequent changing of the blotting paper under the T and the washing of the walls of the T in order that "tracking" should be rendered impossible.

The results demonstrated (a) the superiority of five trials per day over a greater number, and (b) the tendency of No. 2 to follow its own mucous path.

Repeatedly it happened that the worm after being permitted to make several direct trips thru the T, *without change of floor,* made mistakes, hesitated, and wandered as a result of the substitution of fresh blotting paper and the washing of the walls with tap water. But like the other features of the behavior of the worm, the following of the mucous trail laid down in the previous trip is not constant. In many instances the path is wholly disregarded. Only exceptionally, therefore, could a "perfect" series result from the "tracking" tendency. Moreover, since no systematic study of this tendency was made previous to the five-hundredth trial, it is not certain that No. 2 did not acquire its ability to follow its mucous trail. Of chief importance in the present connection is the fact that "tracking" does not suffice to account for the "perfect" series obtained.

On January 26 an important improvement in the technique of the experiment was made by the substituting of the electrical stimulus for the chemical. The salt solution had proved unsatisfactory because each time the worm came into contact with it a certain amount was converged to the adjacent blotting paper. The electrical stimulus proved to be cleaner, more controllable and therefore preferable.

The electrical stimulus consisted of the induced current from a Porter inductorium, with secondary set at 6-7 on the scale, in circuit with a No. 6 Columbia dry battery.

From January 26, 1912, till February 13,—trials 646 to 710—

series of five trials each were given. Only exceptionally did the worm receive the shock in other than the first trial of a series, and in several instances, the series were almost perfect.

At this point in the experimentation is was decided to attempt to break the habit of escaping from the T by the right arm and to substitute for it a habit of escaping by the other arm. Between February 15 and April 9,—trials 711 to 780—five trials per day were given with the sandpaper and electrodes in the right arm, instead of in the left, and with the exit tube at the end of the left arm.

Under these conditions the worm persistently turned to the right, in accordance with its previous training. So strong was this tendency that in the consecutive series of April 6, 7 and 9 not a single correct trial appeared (table 5, left side).

TABLE 5

Results of Attempt to Break up Habit of Turning to Right in T. and Establish the Habit of Turning to the Left

Turning to Left

Date	Trial	Sand-paper	Shock	Time
April 5	761	4	1	200″
“	762	0	0	53″
“	763	5	2	134″
“	764	4	2	260″
“	765	0	0	119″
Av. of five trials		**2.6**	**1.0**	**153.2″**
April 6	766	3	1	248″
“	767	6	3	193″
“	768	3	1	65″
“	769	2	1	107″
“	770	2	1	65″
Av. of five trials		**3.2**	**1.4**	**135.6″**
April 7	771	3	1	149″
“	772	3	3	172″
“	773	2	1	170″
“	774	2	3	145″
“	775	3	1	92″
Av. of five trials		**2.6**	**1.8**	**145.6″**
April 9	776	5	2	173″
“	777	6	1	490″
“	778	4	2	189″
“	779	3	1	151″
“	780	2	1	111″
Av. of five trials		**4.0**	**1.4**	**222.8″**

Turning to Right

Date	Trial	Sand-paper	Shock	Time
April 10	781	0	0	31″
“	782	1	1	67″
“	783	0	0	40″
“	784	0	0	58″
“	785	0	0	30″
“	786	0	0	39″
“	787	0	0	42″
“	788	0	0	44″
“	789	0	0	148″
“	790	0	0	54″
Av. of ten trials		**.1**	**.1**	**55.3″**
April 11	791	0	0	130″
“	792	0	0	54″
“	793	0	0	45″
“	794	0	0	57″
“	795	0	0	45″
Av. of five trials		**0**	**0**	**66.2″**
April 12	796	0	0	97″
“	797	0	0	28″
“	798	0	0	51″
“	799	0	0	36″
“	800	0	0	65″
Av. of five trials		**0**	**0**	**55.4″**

After 70 trials, this attempt to break the direction-habit was given up because of the risk of injury to the subject from frequent electrical stimulation.

Beginning with April 10, the original conditions of training were reestablished, and, with few exceptions, the correct course was taken in trials 781 to 860. Several series were perfect.

The contrast between the features of behavior under the differing conditions of training—with apparatus arranged for escape by the left arm, and for escape by the right arm—is indicated in table 5. With the new condition of training there were few successful trials, with the old there were few failures. With the new, the worm wandered, baffled; with the old it escaped directly.

In the three weeks of training, April 10-30, the tactual and electrical stimuli were seldom received. The worm usually followed the right wall of the stem of the T from the entrance tube and turned to the right with a fair degree of directness upon coming in contact with the "common" wall of the arms.

The results of trials 781 to 860 were more clearly indicative of a definite direction-habit than any previously obtained. Table 6 presents a few typical series. Excellent evidences of the association of tactual with electrical sensations also appeared.

TABLE 6

Results Indicative of Perfection of Habit Before Amputation of First Five Segments

Date	Trial	Sand-paper	Salt	Time	Date	Trial	Sand-paper	Salt	Time
April 23	801	0	0	52″	April 25	821	0	0	141″
"	802	0	0	76″	"	822	0	0	30″
"	803	0	0	45″	"	823	0	0	58″
"	804	0	0	36″	"	824	0	0	37″
"	805	0	0	53″	"	825	0	0	38″
"	806	0	0	40″	"	826	0	0	28″
"	807	0	0	73″	"	827	0	0	35″
"	808	2	0	78″	"	828	0	0	41″
"	809	0	0	49″	"	829	0	0	45″
"	810	3	0	240″	"	830	0	0	48″
Av. of ten trials		.5	0	74.2″	Av. of ten trials		0	0	50.1″
April 24	811	0	0	49″	April 26	831	0	0	61″
"	812	3	0	240″	"	832	3	0	63″
"	813	0	0	30″	"	833	0	0	52″
"	814	0	0	24″	"	834	0	0	49″
"	815	0	0	21″	"	835	0	0	43″
"	816	0	0	40″	"	836	0	0	54″
"	817	0	0	47″	"	837	0	0	72″
"	818	0	0	39″	"	838	0	0	43″
"	819	0	0	62″	"	839	0	0	89″
"	820	0	0	39″	"	840	0	0	281″
Av. of ten trials		.3	0	59.1″	Av. of ten trials		.3	0	80.7″

In view of this positive result of the training, it was deemed worth while to proceed with the next step in the investigation,—namely, amputation of the anterior segments, with the "brain," in order that the relation of the habit to the "brain" might be studied.

At 5 p. m. on April 30 the prostomium, the first four segments and about half of the fifth were amputated with a sharp pair of scissors. The worm was in splendid physical condition at the time. It reacted markedly for a few seconds but not violently to the mutilation. Immediately after the operation it was placed between layers of moist filter paper in a jar. Thus it was left over night.

When uncovered and exposed to the light at 10 a. m., May 1, it immediately began to crawl forward. It seemed very sensitive to photic and tactual stimuli, and it moved freely both forward and backward. The wound, which had closed, appeared to be in excellent condition. After the examination, the filter paper was removed, the jar washed and the worm replaced between layers of fresh paper.

So wholly satisfactory was the condition of the subject on May 2, 40 hours after the operation, that it seemed desirable to resume experimentation.

With the apparatus arranged precisely as in the previous series of trials, (April 30) the worm was permitted to enter the T. It moved forward, more slowly and continuously than before the operation, into the middle of the stem. Thence it proceeded by a path which is reproduced in figure 4. Having reached the "common" wall of the arms, it turned to the left and five times pushed forward to the sandpaper, each time withdrawing upon contact. As it searched with the cut end, for a way of escape, the "tail" became active and moved about as if "feeling" for a path. Shortly a turn toward the right was made and, with repeated attempts to crawl up the glass wall, the worm approached the exit tube. The instant the "head" end came in contact with the moist lining of the tube the worm pushed forward as in "recognition" of the retreat.

The trial which has been described was given at 9:16 a. m., and at 9:30 a second trial resulted in a direct trip thru the T. The note-book record reads: "Entered quickly and directly. With 'head' end raised 3-4 millimeters from the floor and in contact with the right wall of stem it rapidly moved forward to junction of arms. It then veered sharply toward the exit tube. On reaching the "common" wall it stopped and felt about for a few seconds, making no attempt to reach up or to swing from side to side. In this the behavior is unusual. After a few seconds it started for the tube, gradually ascending the wall of the arm as it approached the exit." The trip was a quick one, in which only the unusual features have been mentioned.

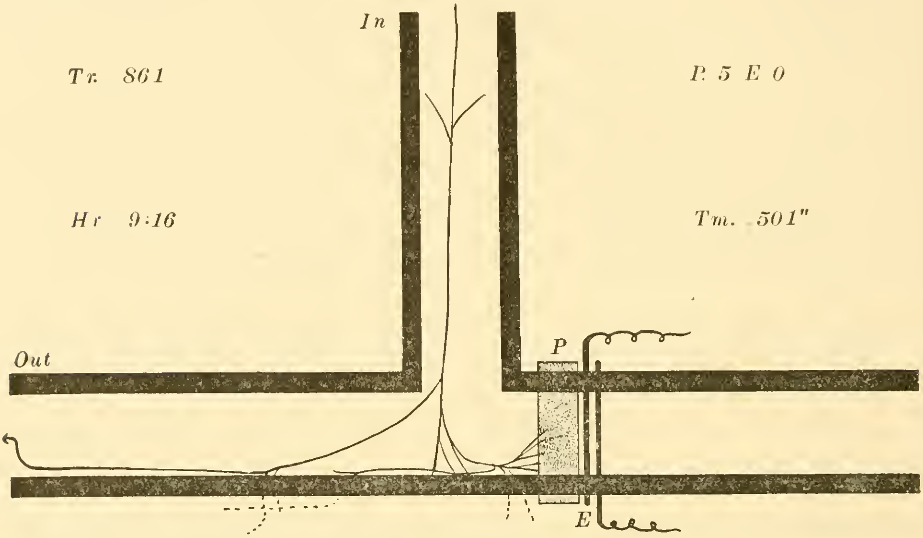

Figure 4. Diagram of T, showing path followed by earthworm No. 2 in first trial after removal of the brain.

The third trial, at 9:52, and the fourth, at 10:03, yielded results similar to that of the second. But in the fifth, 10:15, the path for which appears in figure 5, the worm followed the left wall of the stem instead of the right, felt about at the point of contact with the "common" wall, finally turned toward the exit, and, with frequent attempts to crawl over the wall of the arm, approached, reached, and quickly entered the tube.

Following this first series of trials with the "brainless" worm, at intervals of from two to seven days, additional series of five trials each were given until June 7. The general results appear in table 7.

The most important features of the behavior during this period of rapid regeneration of the "head" will now be described.

In trial 866 (first of May 4) the worm chose the wrong arm. Unmindful of the sandpaper, it pushed forward until the cut end touched the first electrode, whereupon it suddenly drew back and turned toward the exit tube. That this reaction was due to anticipation of the electrical stimulus is possible, but quite as likely it was due to a chemical stimulus from the copper electrode.

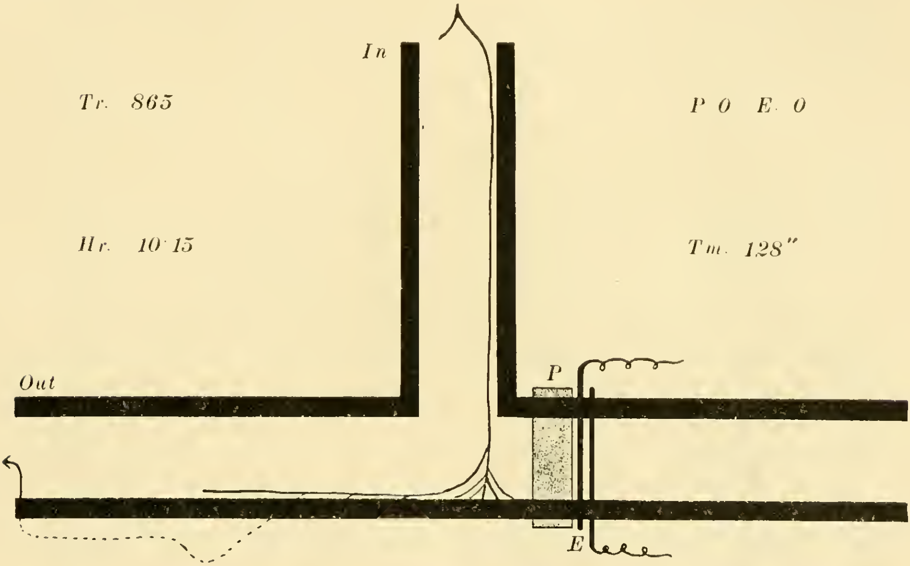

FIGURE 5. Diagram of T, showing path followed by earthworm No. 2 in fifth trial after removal of brain.

In the very next trial (867) the worm turned toward the right as it left the entrance tube, and, coming to the wall of the stem, pushed vigorously against it, while continuing to move forward, as if trying to round the corner of the stem and right arm before reaching it. To the experimenter, it looked as tho it anticipated the turn and miscalculated the distance by just about the length of the amputated segments. As soon as it reached the angle of stem and arm, it veered sharply to the right, while moving toward the "common" wall. Upon reaching the latter it pushed against it, stopped for a moment, then turned to the right again and moved directly and rapidly into the tube.

TABLE 7

Results of Experiments After Removal of Brain on April 30

Date	Trial	Sand-paper	Shock	Time	Date	Trial	Sand-paper	Shock	Time
May 2	861	5	0	501″	May 21	896	1	0	327″
"	862	0	0	91″	"	897	0	0	73″
"	863	0	0	82″	"	898	0	0	55″
"	864	0	0	102″	"	899	0	0	76″
"	865	0	0	128″	"	900	1	0	117″
Av. of five trials		**1**	**0**	**180.8″**	Av. of five trials		**.4**	**0**	**129.6″**
May 4	866	2	0	403″	May 28	901	3	1	300″
"	867	0	0	84″	"	902	0	0	72″
"	868	0	0	201″	"	903	0	0	91″
"	869	0	0	271″	"	904	0	0	48″
"	870	1	0	310″	"	905	0	0	53″
Av. of five trials		**.6**	**0**	**253.8″**	Av. of five trials		**.6**	**2**	**112.8″**
May 6	871	2	1	505″	May 29	906	4	2	355″
"	872	0	0	74″	"	907	0	0	64″
"	873	1	0	411″	"	908	0	0	57″
"	874	0	0	130″	"	909	0	0	39″
"	875	3	2	644″	"	910	0	0	60″
Av. of five trials		**1.2**	**.6**	**358.8″**	Av. of five trials		**.8**	**.4**	**115″**
May 9	876	0	0	149″	June 6	911	0	0	430″
"	877	0	0	126″	"	912	2	0	185″
"	878	0	0	465″	"	913	0	0	77″
"	879	0	0	155″	"	914	0	0	65″
"	880	0	0	231″	"	915	0	0	54″
Av. of five trials		**0**	**0**	**225.2″**	Av. of five trials		**.4**	**0**	**162.2″**
May 13	891	0	0	273″	June 7	916	4	1	442″
"	892	0	0	231″	"	917	0	0	52″
"	893	0	0	107″	"	918	0	0	47″
"	894	0	0	330″	"	919	0	0	45″
"	895	0	0	386″	"	920	0	0	60″
Av. of five trials		**0**	**0**	**265.4″**	Av. of five trials		**.8**	**.2**	**129.2″**

Trial 871, given at 10:45 on May 6, five and one-half days after the operation exhibited precisely the kind of behavior that the experimenter had expected of the "brainless" worm. The subject, with its conspicuous "plug" of regenerating tissue, moved forward readily, but reversed whenever an obstacle was encountered. It wandered about a great deal, as if lost. Finally, it touched the electrodes, received the shock, and reacted violently by "springing back" to the middle of the T. It then reversed and *backed* into the exit tube. The posterior end seemed to "recognize" the tube. Thruout this trial the "tail" manifested unusual initiative.

The worm lost itself in the third, and again in the fifth trial. Concerning the latter the note-book affords the following comments. "The behavior contrasts markedly with that of previous trials. There is little initiative for forward movement. The worm finally had to be driven into the exit tube."

May 6 was evidently a "bad" day, due probably to an unfavorable physical condition.

The worm did not burrow in earth for three weeks after the operation. On May 21, it was found completely covered with loose earth. The regenerated portion at this time appeared clearly segmented and complete, but not yet as large as were the amputated segments.

The tendency to climb the walls of the T which was so conspicuous a feature of the behavior immediately after the operation gradually disappeared as the process of regeneration progressed. Within three weeks, the worm pushed along close to the floor of the apparatus, as it did before the operation.

For the first time, the worm was found naturally embedded in a well-formed earth-burrow on May 28. The regenerated segments were nearly full-size and differed from the others only in their lighter color. On this date the worm wandered more than formerly. It exhibited increased initiative and a proportional increase in mistakes. Whereas immediately after the operation the subject was automatic in its reaction to the apparatus, it now exhibited varied response.

From June 7 until July 4, a period of four weeks, the subject was permitted to rest in order that the persistence of the habit might be tested.

Previously the effect of rest had been observed by the suspension of training from December 19 until January 8, three weeks, with the result that the trials immediately after the "rest" were better than those before it. No indication of loss of the habit by the normal worm within three weeks appeared.

From the outset the trials with the regenerated worm, following upon a four-week's interval of rest, were marked by mistakes. On July 4 not a single trial was correct; on July 5, only two out of five; on July 8, only one of five (table 8).

TABLE 8

Results of Experiments After Complete Regeneration and One Month Without Trials in T

Operation, April 30; *Last Trial, June* 7

Date	Trial	Sand-paper	Shock	Time	Date	Trial	Sand-paper	Shock	Time
July 4	921	1	1	268″	July 16	961	0	0	38″
"	922	2	1	178″	"	962	0	0	44″
"	923	5	1	755″	"	963	1	1	65″
"	924	3	1	461″	"	964	0	0	46″
"	925	9	4	987″	"	965	0	0	53″
Av. of five trials		**4.**	**1.6**	**529.8″**	**Av. of five trials**		**.2**	**.2**	**49.2″**
July 5	926	0	0	113″	July 17	966	3	0	71″
"	927	0	0	54″	"	967	0	0	35″
"	928	3	0	68″	"	968	0	0	50″
"	929	3	2	275″	"	969	1	0	58″
"	930	5	1	668″	"	970	2	0	115″
Av. of five trials		**2.2**	**.6**	**235.6″**	**Av. of five trials**		**1.2**	**0**	**65.8″**
July 8	931	15	13	732″	July 18	971	9	2	257″
"	932	5	8	149″	"	972	0	0	72″
"	933	0	0	117″	"	973	0	0	56″
"	934	3	4	485″	"	974	0	0	57″
"	935	2	1	605″	"	975	0	0	46″
Av. of five trials		**5.**	**5.2**	**417.6″**	**Av. of five trials**		**1.8**	**.4**	**97.6″**
July 9	936	1	1	184″	July 19	976	1	0	241″
"	937	4	2	264″	"	977	0	0	43″
"	938	3	0	49″	"	978	5	2	275″
"	939	0	0	43″	"	979	1	1	94″
"	940	3	1	169″	"	980	4	2	216″
Av. of five trials		**2.2**	**.8**	**141.8″**	**Av. of five trials**		**2.2**	**1.**	**173.8″**
July 10	941	2	0	79″	July 22	981	7	0	217″
"	942	0	0	33″	"	982	0	0	25″
"	943	4	1	106″	"	983	0	0	32″
"	944	0	0	58″	"	984	0	0	42″
"	945	3	2	213″	"	985	0	0	68″
Av. of five trials		**1.8**	**.6**	**97.8″**	**Av. of five trials**		**1.4**	**0**	**76.8″**

Apparently the habit had degenerated. This may be attributed to (a) period of inactivity; (b) regeneration of ganglia and resumption of normal functioning bringing in new modes of reaction; or (c) a combination of these factors.

To the writer it seems probable that the interval of rest witnessed the birth of a new worm thru the regeneration of the nervous system. The habitual reaction which the "brainless" individual had exhibited now gave place to new modes of behavior initiated by the new "brain" cells.

Since it had been demonstrated that the previously acquired direction-habit had disappeared from the regenerated worm after an interval of four weeks' rest, training was instituted for the purpose of re-establishing the habit.

Gradually the behavior changed; the number of mistakes diminished and the trips became more direct. The data of table 8 indicate the nature and rapidity of the change which occurred between July 4 and July 22. Only one mistake was made in each of the series on July 16, 18, and 22.

It would seem therefore that worm No. 2, having (1) acquired a certain direction-habit as the result of systematic training, (2) lost the habit by reason of the regeneration of the ganglia of its anterior segments, and (3) exhibited a tendency to turn in the opposite direction to that demanded in the memory tests, which tendency (4) later was overcome by systematic training and gave place to a definite direction-habit.

SUMMARY

1. The manure worm *Allolobophora foetida*, is capable of acquiring certain modes of reaction which involve a definite direction of movement and the association of two stimuli.

2. The habit appears as a result of from 20 to 100 experiences. It is inconstant even when perfectly formed, varying markedly with the physiological condition of the worm ("good" and "bad" days) and with imperfectly controlled external conditions (temperature, moisture, light, etc.).

3. The results of training on the basis of five trials a day, or every other day, are more satisfactory than those obtained with 10, 15 or 20 trials a day.

4. There is a tendency to "track" (follow the slime or mucous path) if the apparatus is not thoroly cleansed between trials, but this tendency is not sufficiently strong or constant to yield perfect series.

5. Evidences of the effects of experience appear thruout the systematic training in (a) the increased readiness to enter the apparatus and to desert it for the exit tube; (b) the evident "recognition" of the exit tube; (c) the gradually increasing avoidance of the sandpaper, which was meant to serve as a warning against the electrical stimulus; (d) the acquired tendency to avoid contact with the electrodes; (e) the disappearance

of the tendency to attempt to retrace the course thru the stem of the T; (f) the similar disappearance of the tendency to turn back after progressing well toward the exit tube. These several bits of evidence, combined with the still more obvious increase in the number of correct or shortest trips, justify the conclusion that the worm is capable of profiting by experience in a simple maze.

6. The correct performance of a thoroly ingrained habitual act, of the kind studied in this investigation, is not dependent upon the "brain" (portions of the nervous system carried by the five anterior segments), since the worm reacts appropriately within a few hours after its removal.

7. As the brain regenerates, the worm exhibits increased initiative, its behavior becomes less automatic, more variable.

8. Within four weeks after the operation the regenerated segments appear superficially complete and the worm naturally burrows in a mixture of earth and manure.

9. Two months after the removal of the "brain," during the last four weeks of which period no training was given, the habit had completely disappeared from worm No. 2, the subject to whose responses this paper is devoted, and in its place there appeared a tendency to turn in the opposite direction to that demanded in the training.

10. Systematic training for two weeks resulted in the partial re-acquisition of the original direction-habit.

11. The various facts recorded in this investigation indicate that the removal and the regeneration of the first five segments resulted in the development of a worm strikingly different in behavior from the original worm, No. 2.

All of the statements of this paper are based upon the behavior of a single worm, and all of the conclusions are subject to modification in the light of results which are being obtained with other individuals. It has seemed to the writer desirable to present this individual study while the facts are fresh in his mind and the details of the behavior may be used to advantage.

The writer desires to acknowledge his indebtedness to his friend and colleague, Professor Herbert W. Rand, for valuable advice and aid in the investigation.

REACTIONS OF THE MASON WASP, *TRYPOXYLON ALBOTARSUS,* TO LIGHT

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Several organ-pipe shaped cells of this species of mason wasp were placed in an insect cage 16 inches high, 12 inches wide and 12 inches deep. The bottom and the framework of this cage were constructed of wood, the sides and top were covered with fine wire gauze. When not under observation, the cage containing the insects was kept in a room with an average temperature of 70 degrees, and in which the temperature never fell below 60 degrees. The groups of cells [1] were placed close together on the floor of the cage. For food a watch-glass of honey was placed in the cage. To supply the wasps with water, under normal conditions, a cloth syphon led from a bent-neck bottle of water to a Minot watch-glass of mud.

DESCRIPTION OF EXPERIMENTS

SERIES I

In this series of experiments direct sunlight was used as a stimulus. The cage containing the wasps was placed in an east window in such a position that all parts of the cage not shaded by the framework were bathed in the direct rays of the sun. The shadows ranged from two to two and a half inches in width.

While in the sunlight the wasps, as a rule, were quite active, but the behavior was varied. Many climbed the sides of the enclosure; a few flew, at random, about the cage or hovered before one of the sides; occasionally one would sip the honey or drink the water. When wasps in walking upwards through the sunlight arrived at the shadow produced by the upper border of that side of the cage they would usually pass on into the shadow; occasionally one would turn half around and move along the line that separated the lighted from the shaded portion; but in no case did the wasp drop backwards or turn through an angle of 180 degrees.

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[1] For the majority of these nests I must thank my friend Mr. Phil Rau of this city.

Two experiments were performed, one on the seventh and the other on the tenth of June, 1911. In each experiment, at intervals, the position of each wasp was noted. The results of those observations are recorded in the following two tables.

In each of the tables of this article, the number at the top of a column indicates the time when the observation was made. "Li." stands for "In the light," and "Sh." means "In the shadow."

Experiment 1, June 7th, 12 wasps involved.

Wasps resting on or hovering near the	8:30		8:40	
	Li.	Sh.	Li.	Sh.
east side of the cage	5	0	5	2
west side of the cage	0	1	0	0
roof of the cage	0	0	0	0
floor of the cage	2	3	0	2
other two sides	0	1	0	3
Total	7	5	5	7

Experiment 2, June 10th, a. m., from 18 to 28 wasps involved.[2]

Wasp resting on or hovering near	10:30		10:35		10:40		10:45		11:09		11:15		11:30	
	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.
east side of cage	15	3	10	2	8	3	0	4	1	5	6	3	5	3
west side of cage	0	1	0	0	0	1	0	3	0	5	0	5	0	9
roof of the cage	0	0	1	0	0	0	1	0	2	1	3	0	2	0
floor of the cage	5	0	3	2	1	0	1	1	0	4	0	3	1	1
other two sides	0	4	0	5	0	6	0	8	0	6	0	7	0	6
Total	20	7	14	9	9	10	2	16	3	21	9	18	9	19

[2] Wasps resting in or under the mud cells were not counted. Since the number of wasps so situated varied from time to time, the number of wasps in the experiment was not uniform.

SERIES II

In this series the stimulus was direct sunlight. The general arrangement was the same as in series I; but an opaque screen nine inches long and six wide was placed over some portion of the east side of the cage.

The wasps exhibited all of the kinds of behavior mentioned under series I, and, in addition, one was noticed working in the mud, but it did not attempt to construct a nest; and a few of the wasps climbing up through the sunlight were noticed to drop backwards as soon as they reached the shadow.

The following tables record the positions of the wasps at the times they were observed.

Experiment 3, June 7th, 12 wasps involved. Opaque screen covering the upper portion of the east wall of the cage.

Wasp resting on or hovering near the	Li.	Sh.
east side of the cage	3	5
west side of the cage	0	0
roof of the cage	0	0
floor of the cage	0	3
other two sides of the cage	0	1
Total	3	9

Experiment 4, June 8th, 1911, in the forenoon, 14 to 18 wasps involved. Opaque screen arranged across the middle portion of the east side of the cage so as to leave uncovered two inches above it and three below.

Wasps resting on or hovering near	9:37		9:45		10:00		10:05		11:10	
	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.
east side of the cage	3	2	4	3	2	3	6	2	7	2
west side of the cage	0	1	0	0	0	0	0	2	0	3
roof of the cage	0	0	0	0	0	0	0	0	0	0
floor of the cage	5	1	6	0	5	2	4	1	2	3
other two sides of the cage	0	2	0	2	0	2	0	3	0	1
Total	8	6	10	5	7	7	10	8	9	9

SERIES III

In this series of experiments, the cage containing the wasps was placed in a room of twilight brightness and a pyramid of white light from an electric projecting lantern passed through it at intervals. A water cell was used to filter out the majority of the heat rays. On the lantern side of the cage this gave a lighted patch seven inches wide and six high and on the remote side of the cage a similar patch 10 inches wide and eight high. A portion of the roof was also lighted.

No matter how actively the wasps might be flying or walking, when the light from the lantern was shut off they almost immediately became inactive. When the light was turned on, those wasps upon which it impinged immediately became active. Some walked and others flew at random about the cage. At intervals those flying would alight upon some portion of the cage. The positions of the wasps at stated intervals is given in the following table.

Experiment 5, June 14th, 1911, 16 to 25 wasps involved.

Wasps resting on or hovering near the	Lantern light shining						Lantern light turned off					
	11:25		11:44		12:45		11:30		11:35		1:14	
	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.	Li.	Sh.
lantern side of the cage	2	3	4	3	1	6	0	3	0	2	0	8
remote side of the cage	3	0	2	1	0	0	0	1	0	1	0	0
roof of the cage	2	3	1	4	0	3	0	4	0	1	0	3
floor of the cage	0	2	0	2	0	1	0	0	0	0	0	1
other two sides of cage	0	3	0	8	0	8	0	8	0	12	0	5
Total	7	11	7	18	1	18	0	16	0	16	0	17

SERIES IV

In this series of experiments the cage containing the wasps was placed in a room of twilight darkness and, at intervals, pyramids of colored light were projected through the cage, by means of an electric lantern. The sizes and distribution of the light patches were the same as in series III.

Experiment 6. After a beam of white light had been passing through the cage for seven minutes, at 1:22 p. m., a piece of cobalt glass was placed before the lens of the lantern.

The wasps that had been flying actively about the cage suddenly became inactive.

Experiment 7. At 1:37 p. m. a pyramid of white light was passed through the cage used in experiment 6.

A 1:40 p. m. a wasp began to fly and in about a minute several were flying about at random.

Experiment 8. At 1:45 p. m. the cobalt glass was placed before the lens.

By 2 o'clock all of the wasps were quietly resting upon some portion of the cage.

Experiment 9. At 2:03 p. m. the cobalt glass was removed, permitting the white light to pass through the cage.

At 2:04 p. m. a wasp began to fly and in less than two minutes four were on the wing.

Experiment 10. At 2:12 the piece of cobalt glass was placed in front of the lens.

Almost immediately the wasps became quiet. A little later one flew a short distance and then settled down. At 2:15 p. m. two were walking about, but none was on the wing.

Experiment 11. At 11:25 a. m., after the cage had been in twilight darkness for several hours and the wasps were resting upon parts of the cage, a pyramid of orange light was passed through the cage.

Immediately the wasps began flying at random about the cage and hovering before the sides; but, at 11:29 a. m., all had become quiet again.

Experiment 12. At 11:30 a. m. the light was turned out.

Experiment 12. At 11:46 a. m., a pyramid of orange light was passed through the cage.

At first there was practically no reaction. Seven wasps were resting on the sides of the cage with their heads directed upwards, one with its head directed downwards and seven with their heads directed sidewise. Three wasps were resting on the floor of the cage, and one wasp was cleaning its head, wings and abdomen. At 11:50 a. m. one began to fly and one was making a humming noise. In less than a minute after that several were flying at random about the cage, and others were crawling up the sides of it. At 11:55 a. m. all were quiet again, but at 11:56 several were on the wing.

Experiment 13. The light was turned off at 11:58 a. m.

Immediately all became quiet.

Experiment 14. At 12:00 m. a pyramid of ruby light was passed through the cage.

Immediately the wasps became active. Some walked about in an excited manner, others made random flights about the cage and yet others hovered before the sides. On alighting from their flight they would stop in both light and shadow. At 12:03 p. m. two were roaming afoot, the majority were restless, but none were flying.

Experiment 15. At 12:14 the light was turned out.

Soon all the wasps became quiet.

Experiment 16. At 12:16 p. m. a pyramid of ruby light was passed through the cage.

Up to 12:25 p. m., a lapse of nine minutes, all of the wasps remained quiet.

Experiment 17. At 12:25 p. m. a pyramid of white light was projected through the cage.

The light impinged on four wasps. At 12:27 a wasp resting on the lighted patch on the ceiling began to walk about through both light and shadow. At 12:28 p. m. several were making random flights about the cage or hovering before the sides. Two minutes later nine were on the wing.

Experiment 18. At 12:30 p. m. a pyramid of ruby light was passed through the cage.

Immediately the wasps became quiet. At 12:32 p. m. a few walked about and one flew from the top of a mud cell, across the ruby light, to the opposite side of the cage. At 12:38 p. m. all were quiet again.

Experiment 19. At 12:39 p. m. a pyramid of white light was passed through the cage.

In less than a minute five were flying, at random, about the cage.

Experiment 20. A pyramid of blue light was passed through the cage.

The wasps continued to fly.

Experiment 21. At 12:46 p. m. a beam of ruby light was projected through the cage.

Soon all of the wasps became quiet.

Experiment 22. At 12:48 p. m. a pyramid of blue light was passed through the cage.

At once one began to hum; and, in less than a minute, two were on the wing.

Experiment 23. At 12:51 p. m. the light was put out.

The flying ceased at once, a few wasps continued to walk about the cage.

DISCUSSION OF THE EXPERIMENTS AND CONCLUSIONS

The experiments which supplied the data for this paper extended over a period of three weeks; and each day, except Sundays, from two to four sets of experiments were performed. Naturally there were many repetitions of experiments and responses, and many slight modifications of each. These numerous experiments were necessary to secure the data for an impartial analysis of the behavior. To record the details of all of the experiments would be an unfruitful tax upon the patience of the reader; hence only a few sample experiments have been recorded in the section above. The following discussion, however, is based upon all of the experiments that were performed.

Feeding Instincts. Evidently *Trypoxylon albotarsus* is able to instinctively [3] feed or drink from liquids without getting into them. This is evidenced by the manner in which the wasps responded to honey placed in Minot watch-glasses. Almost invariably, whether it was a first visit or a subsequent one, the wasp mounted or alighted on the rim of the watch-glass, fed upon the honey, backed out, made a short flight of orientation, and then flew away. Sometimes the wasp would rest one of its fore feet upon the honey; but more often it would not. Occasionally the flight of orientation was omitted. Only once was a wasp noticed to get smeared with honey. On that occasion the wasp waded through the honey and escaped on the opposite side. After wandering about, partly on foot and partly on the wing, it returned to the honey, and when it had supped, backed away without getting into it.

These wasps also displayed an instinctive tendency to drink water under a variety of conditions. They were noticed to drink from a watch-glass partly filled with mud, from the outside of that watch-glass, and from the cloth syphon that supplied the watch-glass with water. One was noticed to enter the bent-neck bottle, drink and back out. The sipping of the honey and the drinking of the water occurred both in the bright light and in the shadow.

Responses to Brightness. In this species brightness usually serves as a stimulus to activity and darkness or a dim light as a stimulus to inactivity. As a rule, under the influence of a strong white light, these wasps would make active flights or else walk rapidly about. These responses were not tropisms, for the flights were pronouncedly random; there being no fixed relation between the direction of the movement and the rays of light.

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[3] In this article instinct is used to signify inborn responses.

Frequently several seconds, and occasionally a few minutes, would elapse between the impinging of the light rays and the responses of the wasps, and individuals climbing upwards through the sunlight, on reaching the upper edge of the bright patch, often continued to walk about in the shadow for sometime before coming to rest. In each of these cases the behavior might be considered due to the fact that it requires time for the stimulus to overcome the inertia of the nervous system; but, since, under exactly similar external conditions, the reaction times are quite different, certain physiological conditions, of which there were no external evidences, must influence the behavior. Then too, occasionally wasps would be quite active, for long stretches of time, when the external stimulus was a dim light; or else remain inactive, for equally long stretches of time, when exposed to a strong white light. Wasps, when feeding, frequently made no responses to a bright light. In this latter case it is highly probable that the stimulus of the food had a greater effect upon the hungry wasp than the light; but in the other cases there was no external indication of the physiological condition which caused the deviation from the usual type of response.

To ascertain if, in spite of the apparently random flights, *Trypoxylon albotarsus* might not have an instinctive tendency to move either towards or away from the light, at intervals, the positions of the wasps were recorded. Some of these experiments seemed to show conclusively a tendency to move towards or settle in the light (Exp. 4) some seemed to demonstrate an equally marked tendency to move towards or to settle in the shadow (Exp. 5), some seemed to indicate no more tendency one way than the other (Exp. 1). Sometimes, in the same experiment, one portion would indicate that the wasps had a strong tendency to move towards or to settle in the light (Exp. 2; 10:30, 10:35) while other portions indicated an equally strong inclination the other way (Exp. 2; 10:40, 10:45, 11:09, 11:15, 11:30). The only logical outcome of such a dilemma is to conclude that the movements were purely random. That these random movements were for the purpose of escaping from the cage is evidenced by the fact that whenever I made a small opening, whether in the bright light or in the shadow, whether in the side towards the source of the light or in some other side, one or more wasps always escaped or attempted to escape.

Responses to Shadows. In some experiments performed with some parasitic bees [4] which displayed a pronounced tendency to move towards the source of the light it was noticed that bees climbing upwards through the sunlight, on reaching a shadow, almost invariably dropped backwards. Careful observations were made to see if these wasps, which did not exhibit a pronounced tendency to move towards the source of the light, responded in the same manner to shadows encountered under similar conditions. As a rule, when these wasps reached the shadow they either moved on into it or else turned through an angle of 90 degrees and walked along the border line between the light and shadow. In a few cases, less than three per cent of those observed, the wasps dropped back when they reached the shadow.

Responses to Hues. Numerous experiments with colored lights, a few of which have been recorded under series IV, were conducted, not to test the visual powers of wasps, but to ascertain if certain rays stimulated a stronger response than other rays. As to the effect of hues as such the experiments were noncommital. The brightness content, however, seemed to play an important role. When a light stimulus, no matter what the hue, followed one with a lesser brightness content, the wasps usually became active; but when it followed one of greater brightness content the wasps usually became inactive. The orange was the most transparent glass, the blue next and the ruby least. Whether the orange followed twilight brightness, or the blue or the ruby, it usually stimulated the wasps to action. Unfortunately no experiments were made with the orange following a bright white light. When the ruby followed twilight darkness (Exp. 14) usually the wasps were stimulated to activity; but when it followed a bright white light (Exp. 18) or a blue light (Exp. 21) the wasps usually became quiet. When the blue light followed a bright white light (Exp. 6, 8, 10), in a few seconds, the wasps usually became quiet; but when it followed ruby (Exp. 22) the wasps were stimulated to activity. In about 85 per cent of the experiments the responses were as stated above; but the exceptions were too numerous to permit one to predict, with certainty, the response that will follow the use of a certain colored light as a stimulus. I have known the blue to follow a bright white light (Exp. 20) without causing the wasps to become inactive, and I have also noticed other incongruous responses.

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[4] Journal of Animal Behavior, 1911, Vol. 1, pp. 374–392.

When exposed continuously to an activity-inducing stimulus, *Trypoxylon albotarsus* exhibited a rhythm in its behavior. It would be active for a while, take a rest and then become active again. The cause of this rhythm was not apparent. One who has watched these wasps in the open could hardly believe the rest due to fatigue; for the amount of exertion and the time of activity were much less than what is exhibited by wasps afield. The pause may simply have been the result of the failure to find a means of escape. That the wasps were trying to escape is evident; for, when the wasps were actively flying about, no opening could be made anywhere in the cage without one or more wasps escaping or trying to escape.

NOTES

NOTE ON WILDNESS IN DUCKLINGS

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The writer was much interested this year in the behavior, as regards wildness, of certain young ducks hatched under hens. The common, so called "English Mallard" of the English game preserves has long been kept by man and acts at all ages like an extremely tame bird. It is a pure pred Mallard, but how long it has been under domestication is not known. It flies readily.

Not until this year, however, were my pure wild Mallards, *Anas boschas,* induced to lay. The eggs were taken, three nestfuls, placed under hens and hatched. They hatched well, and from the first the young ducklings were extremely tame and easy to manage. They took ordinary feed off the ground, and grew well in competition with other larger ducks in the same enclosure, though at all ages they preferred sunken or floated food. They differed from common ducks as regards their behavior in being much quicker on their feet and livelier in action. From the first they showed great skill and interest in catching insects.

At the same time there were hatched a large number of back crosses between common tame Mallards and pure wild Black Ducks, *Anas tristis.* One of these crosses produced young which were three-fourths *Anas tristis,* while in two other crosses the young were only one-fourth *A. tristis.* The three-fourth *A. tristis* ducklings, although raised under the same conditions as the wild Mallards showed from the first extraordinary wildness. They were extremely hard to manage, did badly under ordinary methods of hand rearing, and required much special care and special feeding. Even then the mortality among them was enormous, while among the wild Mallards it was practically zero. Besides their extreme wildness, the most noticeable trait of the three-fourth *A. tristis* ducklings was their marked aversion to picking food off the ground. Food *had* to be floated on the water pans or submerged. This habit of course, is also commonly seen in ducklings of other wild species, such as Pintails (*Dafila acuta*).

The one-fourth *A. tristis* hybrid ducklings were at first very wild, far wilder than the pure wild Mallards, but they changed with age and became much more like ordinary ducks. They did not show any special aversion to taking food off the ground.

In the wild state *Anas tristis* and *Anas boschas* are very similar in habits and commonly intermingle, *Anas tristis* being more of a coastal bird and having a different breeding area. The different actions of the young under confinement is very interesting, showing in the one bird an adaptability little short of marvellous, and in the other closely related species a condition that would wholly prevent domestication and almost prevent breeding in the pure state. I may say that the female of *A. tristis* very rarely lays eggs in capitivity, though the male crosses quite readily with other species. A control in the behavior of the pure wild Mallards was obtained by a number of young hatched by these same mothers in a secluded inclosed pond. These young are as wild as any wild ducks in the open, and it is rarely that one is able to get even a glimpse of them. *Their wildness then is immediately modifiable in the rearing yard.*

Behavior such as is carried by *A. tristis* certainly seems to be of a markedly dominant character and analagous to that seen in half bred wild rats and hybrid mice between wild and tame stocks.

Whether there is any tendency to a segregation of traits of this sort remains to be studied, but to the writer it does not seem as if this work would be at all easy or satisfactory to carry out among ducks.

SINGING MICE

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In the literature of animal behavior appear several references to the production by mice of sounds of musical quality.

The "singing" of mice is described variously by different writers. Lee[1] states that it consists of a series of chirps at the rate of three or four per second. At the beginning of the series, the chirps are low but gradually they become louder. The "song" of one mouse this author likens to the sweet and varied warbling of a canary. Every note was "clear and distinct."

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[1] Lee, Henry. Singing Mice. *Popular Science Monthly*, 1878, Vol. 14, pp. 102-105.

In referring to the same phenomenon, the naturalist Brehm,[2] attributes the following descriptions to various observers. One informant states that the "song" is an irregular mixture of chirps and trills with here and there a snarling, smacking sound followed by a low murmur. Another describes it as a twitter which is a mixture of long drawn squeaking and piping sounds which may be heard at a distance of twenty paces.

One observer noted the phenomenon only in the case of a female mouse while giving birth to young, while another observer states that only the male sings.

The majority of those who have heard "singing" in mice have assumed that it is due to a diseased condition of the lungs or of the vocal organs, but conditions so diverse as pregnancy and parasites in the liver have also been suggested as causes.

The writer desires to add to the observations already reported an additional record of "singing" mice. About the first of December, 1911, while working one evening in his study, he heard a series of sounds which seemed to come from above the ceiling. At the time, they were thought to resemble the soft chirp of a bird.

Shortly afterward, some wild mice were needed for breeding experiments and, by means of a trap, two mice, a male and a female, were captured in the room.

These animals, while being taken to the Harvard Psychological Laboratory, produced sounds like those previously heard in the room and they continued to do so at intervals after being placed in a laboratory cage.

A few days after their capture, the male escaped. The female was mated with a tame mouse and produced, during the period of observation, five litters, thirty-three individuals. None of these offspring of the mating of "singing" mouse with non-singing individual produced unusual sounds, nor has "singing" appeared either in the second or the third (F_2, F_3) generations.

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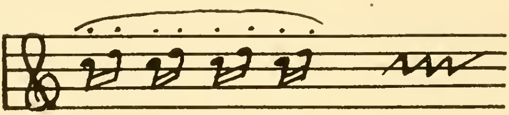
[2] Brehm, A. E. The Life of Animals. Chicago, 1896, p. 338.

Additional references: Shuffeldt, R. W. Natural History of the United States. 1897, p. 420. Ingersoll, E. Life of Mammals. New York, 1906, p. 429. Encyclopedia Americana. Vol. 10, Mouse. New International Encyclopedia. Vol. 14, Mouse. Riverside Natural History. Vol. 5, p. 109.

The "singing" individual was, so far as could be ascertained, a common house mouse (*Mus musculus*). She was somewhat larger than the ordinary wild female, but no other external peculiarities were noted. She was extremely active and savage and her mate always bore the marks of her teeth. An attempt to mate her with a second tame male resulted in the death of the latter.

No definite time for "singing" was noted, except three or four days before and for six or seven days after the birth of a litter. It was observed, also, that the individual "sang" sometimes when frightened.

The sound is best described as a rapid whole-toned trill involving the tones *c* and *d*, as is indicated below.



The quality of the tone resembled somewhat that of a fife or flute, but each tone ended with a slight throaty click. The tones were uttered at the rate of four or five per second in groups of varying size. Sometimes, a group occupied one second, sometimes as long as ten seconds. As a rule, the tones of a group were not clear and distinct but, instead, were uttered so rapidly as to seem connected. The throaty click was more noticeable in the case of the last tone of a group. Often the "singing" would be continued for a period of ten or fifteen minutes with rests between groups.

The sound could readily be heard at a distance of fifteen or twenty feet, but it was difficult to localize it. The individual "sang" little during June, 1912, and it was not heard after July 1st, 1912. She died in August, apparently of old age.

During May, 1912, "singing" was again heard in the room in which the "singing" mice had earlier been captured, but efforts to capture the "singer" failed.

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LITERATURE FOR 1911 ON THE BEHAVIOR OF LOWER INVERTEBRATES

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Protozoa. Two studies of geotropism in microörganisms appeared during the year 1911, both of them tending to support the view that the effect of gravity is rather a mechanical one, like its effects on lifeless bodies, than that of a stimulus to which the organism responds through the irritability of its protoplasm. Harper (13) placed Paramecium in a medium containing very fine particles of iron. When too much iron was ingested by the animals, they sank to the bottom, but when a moderate amount had been taken, there was a tendency to move upward, which Harper ascribes to the fact that the posterior part of the body had had its weight increased by the iron. On the other hand, when finely divided paraffin, a substance lighter than water, was ingested, the opposite tendency was shown. Harper cannot accept the theory of Lyon, that Paramecium orients itself by active movements in response to the stimulus of particles within the body, as in a statocyst, for, he says, "It is not easy to see how an animal revolving continually on its axis could react to the localization of an internal stimulus." The evidence which Lyon obtained for his hypothesis by centrifuging the animals, namely, that under such circumstances the anterior end is directed outward and is therefore probably heavier, so that normal negative geotropism, with the anterior end up, must be active, Harper would set aside by explaining the position of the anterior end as due to the animal's compensatory movement against the rotation. Massart's observations that the positions assumed by dead infusoria are not identical with those assumed in reactions to gravity, as should be the case if the response to gravity is passive, are not mentioned by Harper.

Wager (32) made his observations upon plant forms, such as *Euglena viridis,* Chlamydomonas, Volvox, and Spirillum, but they apply equally to Protozoa. He was especially concerned with the causes leading these organisms to form peculiar aggregations and networks. These aggregations are not related to the presence or absence of carbon dioxide; they are due rather to the forces of gravity and cohesion, "combined with the friction of the water, which sets up a vertical motion as soon as the Euglenae begin to fall." "During the aggregation there is a constant cyclic movement downwards and upwards. The downward movement is a passive one, the Euglenae being orientated into a vertical position with their anterior end upwards." This is because the posterior end is heavier; dead Euglenae sink in the same position. The upward movement is active, but depends upon this same orientation of the body. "The attraction exerted by gravity is not effective unless the Euglenae are present in large numbers. If they are few in number, they are capable of moving in any direction, but always with a tendency to move upwards. The aggregation into networks or groups can only take place when the Euglenae are sufficiently crowded together for the downward pull of gravity to be effective."

The only study of the effect of light on the behavior of unicellular organisms that has appeared during the year, that of Desroche (6), is also concerned with plant life. The zoöspores of *Chlamydomonas Steinii* Goros are found to maintain a constant speed of movement unaffected by the light intensity. The effect of light is not kinetic but orienting, the movements being directed towards the light if it is sufficiently intense.

McClendon (17), Uléhla (31), and Mast (19) have made studies of the motor processes in the Protozoa. McClendon attempts to show the relation of protoplasmic movement to the forces in a magnetic field. Uléhla, by means of the paraboloid condenser, has made a minute analysis of the movements of the flagellum in various species of flagellates. He finds that these movements are seldom those of simple rotation; various figures are described which are peculiar to the species. The flagella have a very complex internal structure, and a very rapid rate of movement. The regularity of their beat is not easily disturbed. Their action is that of oars, not screws. The movements which they describe may be classed under six heads, chiefly dependent on the form and flexibility of the flagella.

Mast's (19) description of the movements of Lacrymaria is known to the readers of this journal. Its most significant content is perhaps the assertion of the random and unoriented character of the movements. The direction of locomotion is regulated by the movements of the head, which seem to be wholly dependent on internal factors. The only evidence of orientation is that "stimulation of the anterior end may not only cause contraction of the neck but also backward movement of the entire organism, while stimulation of the posterior end usually causes forward movement."

The paper by Day and Bentley (5) on learning in Paramecium also appeared in this journal, and therefore needs but brief notice. The method was practically the same as that used by Stevenson Smith, although it was independently developed by Day and Bentley; it consisted in placing an animal in a capillary tube too narrow for it to turn in by the ordinary method of darting backward and turning through an acute angle. A record was kept of the number of efforts it made by this method in successive experiments, before it varied the method and escaped by bending its anterior end. The number of unsuccessful movements and the time occupied by them was reduced in successive trials, showing learning. Precautions were taken against the effects of a chemical change in the medium such as the accumulation of carbon dioxide.

The articles by Jennings (15) and Woodruff (33) are concerned rather with general physiological processes than with behavior. Jennings, however, describes with some minuteness the process of conjugation in Paramecium. The first contact is at the anterior tips, which interlock. The period of "fitting" is one of great and varied activity; there are bendings and contractions, as if the animals were making efforts to bring the mouths together. If they do not fit properly they separate, and thus "assortative mating" occurs. Woodruff notes that if a normal medium is supplied *Paramecium caudatum* can reproduce indefinitely without conjugation or artificial stimulation.

Coelenterates. Schmid (30) finds that in darkness, *Cereactis aurantiaca* withdraws its tentacles and stretches out its body.

When full daylight falls suddenly upon it, the body contracts and the tentacles expand and cover it. In dim light it seeks the lightest region. Yellow and red affect the unfolding differently from green and blue. *Actinia sulcata* drops its tentacles in darkness "as if sleeping."

Echinoderms. The paper by Cowles (3) is chiefly concerned with the reactions to light and shadow of isolated pedicellariae of the sea-urchin *Toxopneustes variegatus.* Von Uexküll has a theory that the reaction of the spines to shadows is due to the release of energy from certain "Tonus" centers in the radial nerve, energy having been stored in these centers by the action of light and being set free when the light is withdrawn. He found that the reaction to shadows did not occur when the connection with the radial nerve was broken. Cowles has observed that in the case of the pedicellariae, however, response to shadowing will take place when they are disconnected from the radial nerve. He concludes that their Tonus centers must lie in the tissue of the pedicellariae themselves.

Annelids. Allolobophora foetida's power of discriminating a wet from a dry surface has been investigated by Parker and Parshley (21). A worm that is creeping over a surface of wet filter paper will, when it comes to a dry spot, after moving over it for a short distance "varying from a few segments to half its length," stop, move its head from side to side, withdraw into the moist area, and take a new direction. When worms were forced to creep backwards into a dry area, they did not show any tendency to check their movements. The authors omit to note that this in itself would not be good evidence as to the part of the body which is especially sensitive to dryness, for the worms might have been so occupied with escaping from the stimulation of the head, which was forcing the backward movements, as to be indifferent to dryness. Individuals with the prostomium removed or anesthetized, however, manifested a similar indifference to dryness and it is concluded that the prostomium is the organ which responds to dryness as a stimulus. This stimulus probably acts through the extraction of water from the peripheral protoplasm of the worm, concentrating and partially coagulating the body materials in the prostomium.

Molluscs. Parker (20) finds that certain snails move by a continuous or arhythmic movement of the foot, instead of by means of rhythmic waves of pedal contraction, as is the case with most species. The foot is an organ of attachment in snails either by means of the mucus it secretes, or by suction, or in both ways. A wave in the foot is a portion that is lifted from the support and is moved forward; this local forward movement takes place in successive sections of the foot until the whole has been moved.

A careful study of the movements of heteropod and pteropod molluscs has been made by Polimanti (26). He has also investigated the functions of the ganglia and statocysts in these animals. Lesion of the cerebral ganglia produces heightened reflex irritability. The pedal ganglion is the organ of locomotion, and when it is destroyed, motor coöordination is lost. Destruction of one statocyst produces rotation towards the uninjured side; destruction of both results in complete disorientation. This is more marked if the eyes are also extirpated. Ordinary sound stimuli have no effect on movement, but jarring the water so as to produce tactile stimulation does have an effect.

Yung's papers (34, 35) report his conclusion that the snail *Helix pomatia*, although it has well developed eyes, is totally blind and not even dermatoptic. The individuals he tested were perfectly indifferent to shade or bright sunlight on the floor of a room, and to dark or light compartments of a box; they did not respond to sudden variations of light intensity such as the turning on of an electric lamp or the flashing of a light by means of a mirror; and they gave no evidence of being able to avoid obstacles by vision. A pin-head was repeatedly approached to the eye and then struck against it: the tentacle bearing the eye was withdrawn at the touch, but never, in spite of repeated experience, before it. Snails with the eyes removed behaved in all these respects precisely like normal individuals. It is certainly interesting to find that an organ apparently in perfectly good condition can be without function. The danger of arguing from purely morphological evidence is well illustrated.

Piéron (24, 25) makes use of the light reactions of a common snail (*limnée*) to investigate the laws of memory. In the first of these two papers he undertakes to find out how rapidly the effects of adaptation to a repeated visual stimulus die away with time. The first step is to determine how many times a shadow of a definite duration, the light being otherwise kept at a given constant intensity, must be made at a fixed rate before the snail will cease to respond by drawing in its tentacles. When this number has been found, the experiment is repeated after a given interval, and if the interval is not too long, the effects of the previous experience will be shown in the fact that not so many repetitions of the shadow are now required to produce cessation of the reaction. The longer the interval, the smaller will be the amount of the saving in repetitions thus brought about. By determining the amount of saving for different intervals, Piéron establishes the formula m (the mnemonic trace) $= \frac{k\ (\log t)\ a}{t\beta}$, where k, a, and β are constants and t is the interval of time elapsing since the first experience. Piéron thinks that the formula thus found is truer even for human memory than that of Ebbinghaus, which is $m = \frac{k}{(\log t)\ a}$, and supports this statement by some experiments on the human subject. In his second paper (25), his purpose is to find out what interval between successive shadowings will most quickly bring about cessation of reaction. He finds that for the fresh-water snail the optimum interval is from ten to twenty seconds, and that for Littorina it is as long as one minute. In man, as has been indicated by the experiments of Jost and others, it may be as long as twenty-four hours. Whether the process of becoming adapted to repeated stimulation really has anything in common with the processes of establishing associations in the human cortex remains a question.

Dawson's monograph on the biology of Physa (4) considers in its first section the subject of habitat. The following conditions are enumerated as those of an optimum environment: "shallow water, minimum amount of shade, few or no enemies, minimum amount of debris, protection from waves and currents, moderate amount of water weeds, and well aerated water." The second section is devoted to mucus and the spinning of mucus threads. The common practice among water snails is, on leaving the substrate on which they have been crawling, to float up through the water, drawing behind them a thread of mucus, on which, after the lung has been filled with air, they descend. The spinning process in Physa is affected by the food supply, by general activity, and by habitat. Its adaptive aspect is not clearly made out. It cannot be related to need of air, for if the snail's lung is not already full of air it will not be light enough to ascend through the water, but must crawl on a solid. In Section III food taking is discussed. Low temperature decreases the snail's eagerness for food. Odorous foods are more quickly found than odorless ones. The snail can sense food at not over one centimeter away. "All parts of the head and the first few mms. of the ventral surface of the foot were found to be sensitive to food." Negative responses were obtained to hydrochloric acid, onion juice, pieplant, and the juice of a walnut husk. When mechanical and chemical stimulation are combined the former is first effective; a snail which in rapid crawling comes in contact with food gives the negative response first and then the feeding response. A well-fed snail has its responses to the neighborhood of food decidedly weakened, while a fasting snail will give the food reaction to mere mechanical stimuli and even to chemical stimuli which ordinarily produce a negative reaction. Part IV, on respiration, contains nothing which calls for report here. The last section is entitled, "Some Psychic Phenomena of Physa." A snail's reaction to stimulation is much influenced by its previous experience, of being handled, for example. A snail dropped into an aquarium where it had been previously kept crawled at once to the surface as though traversing a familiar path, while snails freshly taken from the pond wandered about aimlessly on being dropped into the same aquarium. On travelling several times to the top of the water in an aquarium, the snails showed less aimless wandering and exploratory tapping with the siphon. The following experiment indicates that Physa remembers the location of the surface film. The vessel containing the snails was covered with a glass plate resting directly on the water. The snails came up for air and tapped their siphons on the glass, behaving as they usually do at the surface film. Various other evidences of the influence of past experience are noted.

Crustacea. Most of the work done on animals of this group concerns, in one way or another, their reactions to light. McGinnis (18) has observed the behavior of the fairy shrimp, *Branchipus serratus*, under stimulation by light, heat, and gravity. The animal is positively phototactic under lights from twelve to two hundred and eighty candle power. After exposure to darkness for a period of from twelve hours to six weeks the shrimps are still positive. Light has also a kinetic effect, increasing with its intensity; that is, it stimulates to activity. When Branchipus reacts to light its ventral side is turned towards the source, and its long axis is at right angles to the direction of the rays. The eyes appear to be the only organs for the reception of light as a stimulus. Branchipus is positive to temperatures from 14 to 17 degrees C., and avoids temperatures above and below these limits. It is positively geotropic in light and negatively geotropic in darkness; light therefore must have besides its directive and kinetic effects a third influence whereby geotropism is reversed. It is possible that darkness rather than light may furnish the stimulus to this reversal.

The temperature sensibility of Zoea larvae has been investigated by Schmid (29), who notes a distinct reaction of jumping back given by the animals when they reach the boundary between water at ordinary temperature and water at a temperature too high for them.

The effect of chemicals on the reactions to light made by crustacea has been the subject of some experiments by Bohn and Drzewina. Drzewina (8) finds that hydrocyanic acid is a desensibilisator to light; she notes also that copepods of the plankton are much more sensitive to its influence than those of the surface layers of water, where much decomposition is always going on. Bohn, working with larval lobsters, finds that both acids and alkalis are sensibilisators. The effect lasts longer with alkalis, but is weaker. With acids, the maximum effect is shown at the end of a few minutes, and the effects vanish after a few hours. With alkalis, the maximum effect occurs only after several hours, but the influence may persist for several days. The two experimenters working in collaboration (9) found that potassium cyanide reverses positive phototropism in certain crustacea and "desensibilises" to tactile stimuli more rapidly than to light.

Esterly (10) has reported records of the daily depth migrations of *Calanus finmarchicus*. During June and July the greatest assemblage at the surface of the water is between seven and eight P. M. The migration begins before midnight. Its cause, Esterly thinks, is light, but not directly; rather by the effect of light upon geotropism.

Doubt is cast on the very existence of such depth migrations by Franz in the second of the two papers by him on our list (12). He is strongly inclined to think that biologists have been led to suppose that daily migrations occur from the surface of the water to the deeper regions and *vice versa*, by the simple fact that in daylight the animals can see and avoid the collecting net, whereas at night they cannot. It scarcely seems probable that so obvious a source of error as this should have been overlooked. Franz believes that phototropic reactions are never shown under the influence of light alone, but only when unfavorable life conditions exist, so that the flight-reflex occurs. Whether the phototaxis is positive or negative depends on whether the animal in its ordinary surroundings can escape into the open or into covering. In the first of the two papers on our list (11), he shows that various animals, including copepods and Daphnia, display phototropism only when confined in a small space. In the second paper he makes a similar observation on *Palaemon serratus* and on some larval crustacea and worms. Marine copepods show a varying response to light, some individuals being positive, others negative, and the tropism of a given individual is reversed at times. Franz thinks that on being introduced into the observation vessel, most of them try to escape by swimming towards the light, and that when this procedure is not successful, they become negative. Animals which may display either negative or positive phototropism are those which under normal life conditions can escape in either way. Very young larvae are predominantly positive, owing to their need to seek light. Phototropism being thus dependent on the flight-reflex, "there is scarcely any analogy between phototaxis in animals and phototropism in plants." A final section of this paper considers the peculiar behavior of Hemimysis, which oscillates back and forth in the water in which it is confined, its excursions being from ten to twelve centimeters long. Franz thinks that the turning occurs when the animal gets into a region either too light or too dark for its existing state of adaptation; the reflexes cease when the light is diffused. Foul water makes all the individuals positive; movement from darkness into the open would be their flight-reflex under normal conditions.

Hess also, as is well known, opposes the identification of phototropism in animals and in plants, although on other grounds. He here (41) reports, besides experiments on fish and birds, some tests made with the branchiopod *Artemia salina*, which is normally strongly negative in its response to light. He put a number of individuals into a cubical vessel placed at the middle of a black tunnel; at either end of the tunnel was a light. The sensitiveness of the animals to differences in the intensity of these two lights was measured by keeping one of them at a constant distance of thirty centimeters, and moving the other one. No difference in the behavior of the Artemias was observed until the second light was moved either further off than thirty-two centimeters or nearer than twenty-nine centimeters. When a red light and a blue light were used, a very light red and a very dark blue proved to be equal in their effect on the light reactions of the animals, and corresponding results were obtained by equating red and blue with white light. Another method was used to investigate the reactions of Artemia to colored light. The cubical vessel containing the animals was divided by glass partitions into four parallel compartments, and a spectrum thrown through the side wall in such a way that one compartment was illuminated chiefly by red light, one by orange and yellow, a third by yellow-green and green, and the fourth by blue and violet. The Artemias all crowded against the opposite wall of the vessel. A long, cylindrical incandescent tube was placed parallel to this other side, and the intensity was measured of its light when it was able to overcome the negative effect of the colored light in each compartment. The brightness values of the colors thus measured indicated that they were the same as those for a color-blind human being. Hess believes that he has found the brightness values of colors to the vertebrate eye, on the other hand, to be identical with those for the normal human eye, and argues on this ground against Loeb's position that the effect of light on animals and on plants is essentially the same. His results for the vertebrates do not agree, however, with those of the American investigators.

Pearse (23), by means of careful experiments, described in this journal, on the crayfish, spider-crab, caddis-fly larva, and crab-spider, comes to the conclusion, in opposition to Minkiewicz, that these arthropods are unable to select an environment which matches their own color.

Polimanti (28) describes a case where a crab, *Dromia vulgaris,* had attached to its back a sponge, *Suberites domuncula.* In this case of symbiosis the initiative must be taken by the crab, and Polimanti believes that associative memory plays a certain part in the crab's behavior.

In his study of the mating behavior of certain crabs, Chidester (2) concludes that there is no evidence that sexual selection is involved. The males recognize the females by touch. Males attempted to mate with other males and with fertilized females, but never with individuals of other species. "Opportuneness of proximity" is the most important factor in bringing about mating.

Three papers of a more general character may be briefly mentioned. Doflein (7) asserts his belief in the distinctness of the organs of taste and smell even in water-dwelling animals. The former test substances entering the alimentary canal; the latter test the environment in general. In land crabs the small antennae have an olfactory function, which is doubtless the same in water-dwelling forms.

Polimanti (26) describes observations made in the aquarium at Naples on the periods of activity and repose in a great variety of marine animals, including sea-anemones, hydroids, siphonophores, ctenophores, echinoderms, annelids, crustacea, molluscs, tunicates, cartilaginous and bony fishes, and turtles. In general, he found that periods of great activity in all these animals are followed by short intervals of lessened activity; the repose, however, is never really complete. Temperature and light are the chief regulators of activity and rest. The greatest activity is nocturnal. The comparatively constant temperature of the sea economizes the energies of marine animals and permits their high activity. An abnormal degree of activity often occurs before death.

Lund (16) contributes a brief report of observations on the reactions to light of animals which themselves produce light. Some of the behavior which he studied was that of insects, but among the lower invertebrates he noticed that certain luminous ostracods respond to the stimulus of light by emitting a luminous secretion, and that some of the Cirratulidae and Syllidae are attracted to a source of light and to each other when emitting light. Thus in these forms the self-produced light coöperates with a positive phototropism to bring about reproduction.

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LITERATURE FOR 1911 ON THE BEHAVIOR OF SPIDERS AND INSECTS OTHER THAN ANTS

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TROPISMS

1. *Thigmotropism.* H. H. P. and H. C. Severin (53) have obtained experimental evidence that certain water bugs are positively thigmotactic. In an aquarium containing water one inch deep, they placed thirty-five specimens of *Belostoma flumineum* Say, and scattered on the top of the water several large flat corks. In a few hours thirty-two of the insects were resting against the lower sides of the corks. Frequently, at the surface of the water and at the bottom of the aquarium, two or more Belostomas were found clinging together. This the authors consider a positive thigmotactic response. *Belostoma americanum, Benacus griseus, Nepa apiculata, Ranatra americana,* and *Ranatra kirkaldyi* react in the same manner.

2. *Phototropism.* The opinion has been hitherto held that the Hepialidae fly only at twilight. J. McDunnough (36), however, last summer observed *H. hyperboreus* flying on the northern slope of Mt. Hood between half-past two and three in the afternoon, in the sunlight, on three successive days. He thinks that the species may have changed its habits on account of the coldness of the evenings in that latitude.

According to E. A. Andrews (9) termites, although blind, respond to light by collecting where the intensity is least. Andrews does not call this a tropism, but Bohn would.

If the hand is passed over a jar containing larval mosquitoes, all of the insects swim downwards. In order to classify this behavior, S. J. Holmes (30) has performed a series of experiments. A glass jar containing *Culex territans* and *Culex pipiens* was arranged in a window in such a position that the light fell upon it obliquely. Whenever a dark object was passed over the jar, the larvae swam downwards and towards the side that was away from the light. When conducted in a dark room, with the light impinging on only one side of the aquarium, the experiment yielded the same result. Hence Holmes logically concludes that the direction of the rays of light is a factor in determining the direction of the movements. Whenever a shadow was cast upon a larval mosquito, even when the shadow was cast by an object beneath the aquarium, the larva always moved downwards and away from the light. When shadows were cast upon larvae resting on the bottom of the jar, commotion was produced, but no definitely directed movements. A series of experiments conducted with larvae of all ages demonstrated great variation in their phototactic response, and that the variations are not due to age. Some were indifferent, some moved towards the light, and some moved away from the light; but all responded to shadows in the manner mentioned above. Since the larvae always dart downwards and away from the light, no matter from what direction the shadow comes, Holmes concludes that this characteristic retreat of the larvae is not a tropism, but a specific response to shadows. Evidently this is an example of what Bohn would call differential sensibility, and what less conservative writers would term an exhibition of fear.

According to the same investigator (30), the adult mosquitoes display a peculiar combination of responses to light, but are more apt to settle on dark than on light objects; and during the day they seek the shade even when it is neither hot nor dry. At night they sometimes fly towards the light. The adults of some species show marked positive phototaxis. Eating a full meal does not alter the phototactic response.

C. H. Turner (58) has conducted a series of experiments to test the response of certain parasitic bees of the family Stelidae to light. The bees were confined in a rectangular wire cage sixteen inches high, twelve inches long and twelve wide. To keep them in a physiologically normal condition, they were supplied with honey and with water. Record was kept of their behavior under the following conditions: when all sides of the cage were exposed to diffuse daylight, when one side of the cage was exposed to the oblique rays of direct sunlight, when the top and two sides of the cage were exposed to such rays, when such rays were admitted only through the upper or lower half of one side, when a narrow beam of electric light was passed through the cage in such a manner as not to impinge upon any of the inmates, etc. The experiments demonstrated that, when confined within a small cage, the bees of the species studied make certain movements that finally bring them in contact with that side of the cage through which the direct rays of the sun are entering. That this effect is produced, not by a bright patch, but by the direction of the rays of light, is evidenced by the fact that a strong beam of light may be projected through the cage without arousing any response from bees upon which the light does not impinge. A shadow cast upon a bee basking in the sun usually causes it to move. The aroused bee may go to a bright spot on the cage, or to the bottom of the cage, or to the mud cell that is its home. Frequently the bees crossed the rays of light at every possible angle and examined first one side and then another of the cage, as though seeking an exit. These experiments caused Turner to conclude: "These bees are endowed with a pronounced tendency to move in the direction of the rays of light and towards their source; but they do not invariably so react; frequently they make unpredictable responses. Light, heat, sexual restlessness and, perhaps, other factors arouse in these bees an impulse to roam from home. Coupled with this impulse to roam, there is an instinctive tendency to seek freedom in the direction of the rays of light. When following this instinctive tendency fails to bring freedom, the bee tries other methods. In this endeavor many bees make haphazard flights in all possible directions; while others, in a more systematic manner, hover repeatedly before the sides of the enclosure. If such behavior can be called a tropism, then these bees are positively phototactic. Without predicating curiosity or logical thinking to these bees, and granting that the behavior of a boy is more flexible, it seems to the present writer that the reaction of these bees towards light resembles more the response of a small boy to the music of a brass band than it does the turning of a magnetic needle towards the pole."

SENSATIONS

1. *Auditory Sensations.* E. A. Andrews (9) has conducted some experiments which he thinks indicate that termites respond to concussions of air as such. A nest of termites was suspended from the ceiling by means of a copper wire. In a pan of water supported by a concrete wall extending from the ground, he placed an artificial stone weighing sixteen pounds. The nest

communicated with this moist stone by means of a long stick of wood. Thousands of individuals from the nest frequented this stone. The noise of thunder or of the blasting of rocks would send the insects hurrying to the nest. Even the clapping of hands, which was probably too slight to jar the stone, would produce the same effect; but the blowing of horns at various pitches produced no effect. Andrews does not claim that this is a case of audition; he simply holds that the termites seem to respond to certain concussions of the air.

2. *Sense of Smell.* Andrews (9) thinks that many of the activities of termites are influenced by stimuli which produce something akin to the sense of smell. (See Andrews under "Homing").

3. *Vision.* During the year two experimenters, Allard (8) and Turner (57) have conducted experiments on the color vision of insects. The methods and the materials used were different, yet each investigator is convinced that the bees possess true color vision.

Allard (8) worked with various bees (*Melissodes bimaculata*, Bombus, Entecnia, Apus). He used the following objects as stimuli: normal cotton blossoms, and blossoms whose petals had been removed; cotton blossom petals pinned to the stem, a single petal thus pinned on, cloth petals arranged to resemble a cotton blossom, cloth petals so arranged and covered with real cotton petals, and, finally, cotton leaves wrapped around petals to imitate a cotton bud. These objects were arranged in groups of threes on a cotton field, the grouping being either in a triangle or in a straight line. The results of Allard's experiments lead him to the following conclusions: bees are guided to cotton blossoms by sight, after they have entered the cotton field; the inspecting process, however, is not dependent for its initiation on the size and general appearance of the blossoms; if a blossom is visible only to bees directly above it, it is seldom visited; artificial objects are rarely inspected, probably because the bees observe their differences in color and texture from the natural objects; a blossom is inspected much oftener than it is entered; it is hard to determine the relative importance of sight and smell in determining close inspection of a blossom, although sight is instrumental in bringing about the first approach; bees are much influenced by conspicuousness and coloration in perceiving cotton blossoms; associative memory of a kind is involved, since young bees do not appear to work to so much advantage as experienced bees. Experience is also evident in the fact that bees which have been collecting honey from American cotton blossoms, which have extra-floral nectaries, visit similar structures on blossoms of the Asiatic variety, which have no honey in them; but soon depart. The cotton field itself, Allard thinks, is found by a kind of odoriferous cloud hanging over it. He does not bring any experimental evidence to the support of this statement.

C. H. Turner (57), in experimenting on pattern vision in the honey bee, made use of pasteboard boxes like those employed in his experiments of last year on color vision in the same insect. He constructed artifacts showing seven different color patterns. Bees which had been trained to gather honey from one pattern were tested to see if they could choose this pattern from one or more of the others. In some cases the artifact of the pattern to be chosen contained honey, while the rest had none; in other cases there was honey on some of the artifacts of all patterns, and in still other cases there was no honey on any of the artifacts. Five hundred and eight correct selections out of 518 were made, indicating that color patterns are perceived by bees. Since they can distinguish both color and pattern, no evidence can be drawn from the visual powers of bees against the hypothesis that colors and patterns in flowers are adapted to secure the visits of insects.

The experiments discussed above do not demonstrate that colors appear to insects just as they appear to us; that question cannot be determined experimentally; but, it seems to me, they do prove that these insects discriminate between colors. Furthermore, these experiments do not predicate to insects color preference. Color discrimination is one thing and color preference another. Recently A. S. Pearse (43) has given some attention to the matter of the color preferences of insects. His experiments were planned to test the ability of the larval caddis-flies (*Neuronia postica* Walker) and of the crab spider (*Misumena aleatoria* (Hentz) Emerton) to select from a variety of colors those that match their environment. The caddis-fly larvae were placed in small rectangular aquaria and each aquarium placed in a tightly fitting box, the inside of which was painted a certain color. (This box was called a color box). A piece of card-board the same color as the inside of the color box was arranged above the aquarium in such a manner as to reflect that color into the aquarium. The colors used were black, white, yellow, green, blue. He then cut a number of one by fifteen millimeter paper slips that were so colored as to match the inside of the color boxes. Four slips of each color were placed in each jar. In selecting slips to build into their cases, the larvae did not display any color preference. At the beginning of his experiments on the spiders, Pearse tested the spiders' reactions towards a beam of white light and demonstrated that the spiders do not exhibit any form of phototropism. Other preliminary experiments demonstrated that although these spiders occasionally change in color until they resemble the environment, yet the change takes place too slowly to be of any protective value. The real tests were of two kinds: experiments with color-boxes and experiments with flowers. In nature some specimens of the species examined are white and others are yellow. One vertical half of each color box was painted white and the other half yellow. By means of a small opening, the spiders were passed, through a narrow tube, into the box at the place where the two colors met, and a record kept of the side into which the spider went. In the flower experiments, a spider was suspended, by means of a fine thread, half way between a yellow and a white blossom, and a record made of which way the spider attempted to go. Neither type of experiment gave any evidence that the spiders exhibited color preference. Pearse experimented with crustacea as well as insects and, as a result of all his experiments, he concludes: "It cannot at present be affirmed that any protectively colored arthropod reacts towards colored objects or backgrounds in such a way that it can be said to have even an instinctive knowledge that it is protectively colored; *i.e.,* arthropods do not choose the most favorable color environment on account of color." The experiments warrant the conclusion; but it must be remembered that proving that certain insects do not exhibit color preference does not disprove that insects possess color discrimination.

FEELING AND EMOTIONS

It has been found by H. H. P. and H. C. Severin (53) that when an aquarium containing Belostomas is suddenly approached, the bugs flee in all directions from their resting places, a performance which is considered by these investigators as indicating fear. *Nepa apiculatum*, *Ranatra americana* and *Ranatra kirkaldyi* show no such signs of fear under similar conditions. At the present stage in the development of the science of animal behavior, it is probably impossible to state what would be considered a conclusive attitudinal indication of fear. A more conservative writer would be inclined to call the behavior of these bugs an example of differential sensibility.

A. D. Hardy (26) thinks he has discovered in the wasp *Diamma bicolor* signs of anger. He bases his conclusion upon the following observation. Diamma was dragging a cricket into her burrow when Hardy, with a pair of forceps, held the cricket by the hind legs. After tugging and tugging without accomplishing anything, the wasp suddenly stopped pulling, mounted the cricket, seized its abdomen with her jaws and stung the insect three times. A reviewer would hardly be warranted in asserting that this is not a case of anger; but it does seem that another interpretation is possible. At another place in the article Hardy has described what the wasp does when an insect recovers from the sting and begins to move. In that case the wasp stops tugging, mounts the cricket, seizes its abdomen in her jaws and stings it. A paralyzed cricket would be unable to resist the efforts of the wasp to drag it along. A cricket that had wholly or even partly recovered from the effects of the sting could, by clinging to the surroundings, offer such resistance, and hinder the wasp in the same manner that Hardy did with his forceps. Until it has been proven that the way in which the wasp reacted to the cricket held by Hardy is not its normal manner of reacting to a revived victim struggling to escape, it does not seem necessary to predicate anger in order to explain the behavior described by Hardy.

MATING INSTINCTS

According to J. B. Davey (20) the tsetse fly (*Glossina fusca*) mates on the trunks of trees.

F. Fuchs (22) noticed two males copulating with one female *Cheimatobia brunnata*. The next morning both males were dead.

The female laid eggs. He also observed a male *Larentia bilineata* copulating with a female *Acidalia aversata*. Next morning both were dead; no eggs were laid.

Hinds and Turner (29) find that the rice-weevil (*Calandra oryza* L.) may mate within twenty-four hours after emergence, and that it is both polyandrous and polygamous.

E. N. Cory (18) describes in detail the copulation of *Sanninoidea exitiosa* Say. He finds that copulation always occurs on some support, and that a pair remains *in copulo* from 51 to 82 minutes.

A. F. Burgess (15) states that the beetles of the species *Calosoma sycophanta* copulate several times during the summer. Between June 27 and July 26, one pair was observed to copulate thirteen times.

Burgess (15) placed a trap cage containing adult females of the species *Calosoma sycophanta* in the open, half a mile from any trees. No males entered the trap. A trap cage containing female *C. sycophantas* was placed in an open space at least two miles from any males. Males were liberated at distances of one half mile, one mile, etc. No males entered the trap.

C. H. Turner (58) describes in detail the mating of a parasitic bee of the family Stelidae. These bees were noticed to mate in both the sunshine and the shade; but always on a vertical or horizontal support, never on the wing.

That certain syrphid flies hover before flowers for long stretches of time, and that several other species of flies engage in a kind of a dance, in which the heads of all of the flies are directed the same way, has been known for several years. By some people the first phenomenon is considered evidence that these flies possess an aesthetic taste; by entomologists the second is usually considered an anemotropism. A desire to obtain a rational explanation for these two types of behavior caused Pérez (46) to make a careful study of the hovering of one species of fly and of the aerial dances of several. He noticed that *Syrphus le balteatus* Degh. hovers in practically a stationary position before a flower or object for a while and then suddenly darts to one side or to the other. Plateau noticed that the hand can be interposed between the fly and the flower without the fly's giving any indication that it notices the substitution. He also observed that if the hand be moved to right or to the left, forward or backward, the fly will move in the same sense. By this means Plateau succeeded in inducing a fly to move almost a meter from its original position. This work of Plateau convinced Pérez that it is not the beauty of the flowers that causes the flies to hover before them. Pérez soon noticed that the flies hovering before the flowers were always males, and that the darting sidewise was initiated by the arrival of small insects. Suspecting that the male was lying in wait for a female, he captured a female of the same species and released it near the hovering male. At once the male darted after it. Although the copulation was not observed, Pérez thinks mating occurred. According to Pérez, *Homalomyia manicata* Meig., *Homalomyia la acalaris* F., *Anthomyia pluvialis* L., and *Anthomyia albicincta* Fall. engage in complex dances in which the flies rise and fall in vertical planes. None but males participate in the dances. When a female appears and a male succeeds in reaching her, he abandons the dance. If a number of females are turned loose in the midst of a number of dancing males of *H. maniculata,* in a short while all of the dancers will disappear. *Limnibia chorea* Meig. has a complicated dance in which the male flies move in horizontal rather than vertical planes. Neither *Calliphora erythrocephala* nor *Lucilia caesar* engages in dances. In each of these species the male rests quietly on some support until a female comes within range, and then darts after her. In *Chloria demandata* Fabr. males and females walk about on some isolated surface. When a male approaches a female, he describes circles about her. After circling in one direction for a short time, he turns about and moves in the opposite direction. After this has been repeated several times the flies mate. These experiments caused Pérez to conclude that both the stationary hovering of the Syrphidae and the aerial dances of other flies are means of securing mating. This seems to be the most logical interpretation of this behavior that has been proposed. The hovering of the Syrphidae in one place, the aerial dances of certain flies, the posing of others on surfaces and the sun-dances of certain bees are all brought under the head of what has been termed a "nuptial ambuscade." *

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* C. H. Turner. The Sun-Dance of Mellissodes. *Psyche,* 1908, 122-124.

NEST-BUILDING AND MATERNAL INSTINCTS

According to Glen Herrick (27), in October and early November the fertilized female of the cabbage aphis lays a large number of eggs on the cabbage. A few eggs are laid on rape, turnips, brussels sprouts and kohl-rabi. Usually the eggs are placed in depressions and crevices on the underside of the leaf. These eggs hatch the following spring.

Hinds and Turner (29) state that the rice-weevil deposits its eggs in the kernel of some grain. The eggs hatch in about three days after they are laid.

According to H. H. King (31), the eggs of *Tabanus par* Walker are laid on the under sides of the leaves of water plants. Although in a cluster, each egg is vertical and distinct.

E. N. Cory (18) states that *Sanninoidea exitiosa* Say, when about to oviposit, arches the center of her abdomen upward and points the tip, with its protruding ovipositor, downward. She gently swings the abdomen from side to side, occasionally pausing to touch it to the leaf. Each egg is deposited singly and glued to the leaf by a secretion which is placed on the leaf before the delivery of the egg. Of 455 eggs deposited, 390 were placed on leaves and 65 on the trunk.

According to E. R. Sasscer (51), the ash saw-fly (*Tomostethus multicinctus* Rohwer), in ovipositing, points her head downward and thrusts her ovipositor under the epidermis near the edge of the leaf and, after laying an egg, flies to another leaf and repeats the process. The egg is always inserted near the edge of the leaf and not in the petiole nor in the midrib, as is done by closely related species.

P. J. Parrott (41) confined three species of tree-crickets (*Oecanthus niveus* DeGeer, *O. nigricornis* Walker, *O. quadripunctatus* Beut.) in breeding cages, with growing apples and raspberries. He used ten pairs of *niveus*, six of *nigricornis* and four of *quadripunctatus.* When apples and raspberries were both present, *niveus* always oviposited in apples; when apples were not present it used raspberries. *Nigricornis* and *quadripunctatus* always preferred raspberries; when it had no other alternative, *nigricornis* sometimes laid in applewood. In ovipositing the female gnaws a hole in the bark; then, moving the ovipositor at right angles to her body, she advances, inserts the tip of her ovipositor into the hole, and proceeds to drill a cavity for the reception of the egg. The drilling is accomplished by thrusting and rotating the ovipositor. After ovipositing, an adhesive substance is discharged, which is kneaded about the egg by the ovipositor. After withdrawing her ovipositor, the cricket reverses her position and completes the process by capping the opening with small pieces of bark, which are held in place by the mucilaginous substance surrounding the egg. The time required for the process varies with the species, and, to a lesser extent, with the individual. In apple-wood one and one-half inches in diameter, *O. niveus* spent seven to sixteen minutes gnawing the hole, twenty-seven to seventy-six minutes drilling with her ovipositor and three to five minutes depositing and disguising the egg. In ovipositing in raspberries, *O. nigricornis* spent five to eleven minutes gnawing the bark, twelve to seventeen minutes boring with her ovipositor and five to seven minutes laying and disguising the egg.

In the summer of 1908, Paul Lozinsky (33) found, in a semi-dark place, a glass tube 147 mm. long and 7.5 mm. in diameter, which contained a nest of *Osmia bicornis* L. The nest consisted of nine full cells and one which had been sealed empty.

The larvae of *Mecistogaster modestus* have been observed by P. P. Calvert (16) living in water between the leaves of epiphitic bromelids. This mode of life Calvert thinks originated in the accidental laying of eggs in the bromelids at a time when the level of the water had been raised by floods. After the plant is again far above the water the association still persists.

E. A. Andrews (9) writes that the chief areas of termite activities are the nest, the arcades and the feeding grounds. The arcades are closed archways that extend from the nest for a distance of about fifty feet. Through these arcades the termites move to and fro in groups. No description is furnished of the construction of the nest; but a detailed description is given of the method of constructing and repairing the arcades. There are four steps in the construction of an arcade: establishing the path; adding to the path, at irregular intervals, sections of two parallel walls; filling in the gaps in these walls; adding a roof to the walls. The wall is constructed of vegetable matter that is cemented with anal discharges. A female adds a mouthful of material; then, turning, cements and glazes it with a fluid which she squirts from her anus. In some cases coral sand is added to the outside. Often there are chinks in the walls. Whenever the arcade warps from the support, the termites construct a new floor. Any punctures that occur in the wall are repaired, from the inside, by anal secretion added drop by drop.

C. H. Turner (59) has noticed an American Ammophila stock its nest with subterranean caterpillars. That European Ammophilas stock their nests with subterranean caterpillars has long been known; but it is thought that this is the first time such a habit has been recorded of any of our American species.

E. Roubaud (49) has had the unusual opportunity of observing, within a single genus, various stages in the evolution of an instinct. The Synagris are a genus of Eumenidae living in the Congo region. *S. calida* L. constructs a nest of several mud cells, stocks the cells with caterpillars, lays an egg in each, seals the cells and takes no further notice of them. *S. sicheliana* Sauss. lays an egg in each of several mud cells, places in each enough caterpillars to last the larva a little more than one day, and replenishes the store daily. *S. cornuta* L., when she has completed one cell, lays an egg in it; but does not stock it with caterpillars. When the larva has hatched, it is fed by the female wasp until it is a full grown larva. Then the mother seals the cell and proceeds to construct another.

FIGHTING AND FOOD-PROCURING INSTINCTS

According to the Severins (53), Belostoma is carnivorous. It captures its prey with its front legs and holds it with them until the juices have been sucked from the body. It feeds on back-swimmers, water-boatmen, larval and nymphal dragon-flies, snails, may-flies, and larvae and weaker specimens of its own kind. Nepa also is carnivorous, but it never runs nor swims after its prey.

C. C. Gowdey (1) observed two specimens of *Bembex tricolor* Dahl. carrying off an example of *Tabanus secedens* Walk.

According to R. Boyce (14), the natural food of *Stegomyia fasciata* is minute algae. He finds that this mosquito bites by day as well as by night.

H. M. Bower (13), while studying the early stages of *Lycaena lygdamus* Doubleday, noted several cases of cannibalism.

N. Banks (10) noticed a phorid fly, *Aphiochoeta xantippi* Banks, attacking a myriapod (Parajulus sp.?).

F. Knab (32) discovers that Megarhinus, a genus of mosquitoes, does not suck blood, but feeds upon fruit. And he thinks that all authors who believe otherwise base their opinion upon insufficient evidence. Although all parts of the proboscis of the blood-sucking forms are present in the proboscis of this form, yet the sheath or labium is strongly chitinized and rigid, which is not the case in the blood-suckers. These forms have been observed feeding upon honey.

Chas. H. Davidson (21) describes a case of a fish (*Lucius americanus* Gmelin), which was nearly three and five-eighths inches long, being captured and fed upon by a large bug (*Lethocerus* (*Belostoma*) *americanus* Leidy).

F. A. McDermott (35) noticed a larval half-winged bug attack a tent-caterpillar. The bug thrust its beak at the caterpillar several times before it succeeded in piercing the skin. At each thrust the caterpillar moved its head towards the bug and the bug dodged. When once the skin of the back had been pierced, the caterpillar offered no further resistance.

W. C. Coker (17) has conducted some simple but conclusive experiments to test the ability of the house-fly to do without water and without food. He placed a certain number of flies in dry tumblers and an equal number in tumblers kept moist by damp sphagnum moss. At the end of forty-eight hours all of the flies in the dry tumblers were dead; those in the moist chambers were living, but not so active as at first. At the end of seventy-two hours all of the flies were either dead or too feeble to walk. Two weeks later this experiment was repeated with practically the same result. Evidently, under normal conditions, the flies investigated cannot live more than about two days without water and not more than about four days without food.

E. A. Andrews (9) notes that the termites feed chiefly upon wood. He did not find any true "fungus gardens." In their stead he found dry masses of stored-up food, which Andrews thinks may represent the primitive forms from which the true culture gardens of other climates have been developed.

In order to see whether the sarcophid flies are parasites or scavengers, T. L. Patterson (42) conducted a series of experiments.

From an area with a radius of ten miles, 3,273 pupae and prepupal larvae were collected and placed in pasteboard boxes 8x5x4 inches. These boxes were examined almost daily. No sarcophagids were raised from any of the active pupae or prepupal larvae; but, from the dead and inactive pupae many were hatched. Live prepupal larvae and active sarcophid flies were confined to cages for several days; after which the larvae were transferred to jelly glasses with mosquito-netting tops. In no case did a sarcophid develop. Live maggots of these flies were placed on active pupae and prepupal larvae, but they did not enter the bodies of any; nor would adult flies oviposit upon freshly killed material. When badly damaged larvae were placed in cages with sarcophid flies, the flies oviposited on them. These experiments demonstrate that the Sarcophagidae used for these experiments are not parasites but scavengers.

MISCELLANEOUS INSTINCTS

Migrations. M. D. Hill (28) describes a case of dragon-flies migrating in swarms.

A moth (*Tortrix fumiferana* Clemens) in July, 1911, entered Philadelphia in such swarms that traffic was interfered with and shop keepers were obliged to close their doors (2).

Hibernation. In the South, according to Glen Herrick (27), the cabbage-aphis (*Aphis brassicae*) hibernates in the agamic form.

According to Burgess (15), *Calosoma sycophanta* hibernates at from two to twenty inches below the surface of the ground. Males and females emerge from the ground at the same time. This statement is based on a study of 734 cases.

Locomotion. According to the Severins (53) Belostoma swims rapidly and Nepa slowly. In swimming both use only the middle and hind pairs of legs and with these they make alternate movements. In walking on land all of the legs are used by both genera.

In testing how far scale insects can walk without feeding, Quayle (48) traced the paths of specimens which were allowed to walk across large sheets of paper. He studied the black scale (*Saissetia oleae* Bern), the red or orange scale (*Chrysomphalus aurantii* Mask.) and the purple scale (*Lepidosaphes beckii* Newm.). The rate of locomotion varied with the temperature. The maximum distance traveled by the black scale was four feet.

DIVISION OF LABOR

E. A. Andrews (9) asserts that each caste of the termites has its special work to do. "The workers do all of the mechanical work; bite off and transport the wood; feed the soldiers and males and females; clean the males, females and soldiers; remove the eggs from the orifice on the end of the female and clean and transport them; do all the work of arcade and nest building and all the biting in defense of the community. The soldiers do no mechanical work, except to move themselves; they appear first when the nest or arcade is disturbed, they explore what is novel, lead the advance of processions, stand placed like guards along the side of processions. They respond to anything unusual rather by exploratory advances than by retreat, being the quicker moving and more responsive members of the community. They form the "investigator rather than the soldier caste. Yet they fight, in a refined way, by ejection of a secretion that binds the enemy fast." There is no overlapping of the functions of one caste by the functions of another such as Miss Buckingham* found to be true among ants.

HOMING

Hardy (26) noticed a *Diamma bicolor* dragging a cricket to her burrow. The wasp was moving backwards. Often she stopped to reconnoiter; but always continued in the same general direction. When the cricket was removed by Hardy from the place where it had been deposited and placed six inches to one side, the wasp became very much agitated and made a random search, going over the same ground several times. At one time the cricket partly revived, and became somewhat active. The wasp stopped her tugging and stung it again.

Andrews (9) found that a small object placed across the trail of termites would cause both the outgoing and the incoming individuals to halt when they arrived at it. Finally they would construct a spur around the obstacle to the trail beyond. This caused Andrews to conclude that the termites are guided by a sense akin to smell. He thinks they are also partly guided by a sense of direction; but can hardly be said to have established this last point experimentally.

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* Buckingham, E. N. Division of Labor Among Ants. *Proc. Amer. Acad. of Arts and Sciences*, 1911, **16**, 425-507.

SOUND-PRODUCING ACTIVITIES.

During the year, Allard (3-7) has produced several papers on the stridulations of the Orthoptera. In one (4) he gives a general discussion of the subject and describes methods of studying this form of behavior; in the other papers (3, 5, 6, 7) he discusses in detail the sounds produced by a large number of species. The statements in the remainder of this paragraph are condensed from the paper (4) containing the general discussion. Stridulating powers are highly developed in the Acridiidae, Locustidae and Gryllidae; the last mentioned being the most musical and the first the least. Strident sounds of insects are always of an instrumental and never of a vocal sort. Usually musical sounds are entirely absent; when present they are usually of a continuous monotonous tone. Except some Acridiidae, which stridulate by movements of the hind legs against the tegmina, the stridulations of the Orthoptera are produced by definitely controlled movements of the tegmina, which have been modified for that purpose. Among the Acridiidae that stridulate during flight the inner wings are brought against the tegmina at will. Among the more highly specialized Locustidae a portion of the base of the tegmina has been modified for sound production. The wings of the Gryllidae show the most complete modification. Entomologists have neglected to study the musical habits of the Orthoptera, hence the stridulations of only the common forms are known. Attempts to reduce the sounds to music have not been wholly successful, because the sounds are usually unmusical. All of the Gryllidae, however, have true musical tones. Slight changes in the light, temperature, and moisture have peculiar, but specific, influences upon the character of the stridulation. Forms that in summer stridulate only during the night, in the cool fall often stridulate only during the day. In the summer time, the atmospheric condition preceding a thunder storm at night causes many Orthoptera to be musical until the storm has passed. Marked differences in stridulation may characterize certain species in different parts of their range; this is especially true of *Gryllus pennsylvanicus*.

C. Pemberton (44) has described the stridulation of the shield-backed grasshoppers *Neduba* and *Aglaothorax*. In these genera the elytra of the males are highly specialized for sound production.

G. T. Lyle (34) records the stridulation of the pupa of an ichneumonid.

Two beliefs are extant as to how sounds are produced by the Diptera and by the Hymenoptera. Landois, who has been regarded an authority, held that, in addition to a sound made by the wings, flies, gnats, bees and wasps produced a finer sound, by means of an apparatus connected with the spiracles and tracheae. Pemberton (45) has conducted a series of experiments to test the validity of this view. In those experiments he used the bumble-bee, the honey-bee, the house-fly and several species of Syrphidae. When a fly was held by the legs in such a manner as to permit free movements of the wings, two sounds were heard; one of low pitch, caused by the vibration of the wings, and one of high pitch, caused by the bases of the wings striking against the body. When a fly, the wings of which are reduced to stumps, is held by the legs, a single sound of high pitch is heard. If the stumps of the wings be touched or if they be glued to the body, no sound is produced. If the thoracic spiracle be completely destroyed by means of a needle, and the abdomen cut off, if the wings are left free, a high pitched buzz of short duration is produced. When the wings are pulled from the fly in such a manner as to leave no stubs, no sound is produced. These experiments caused Pemberton to assert that there is no spiracular voice in either the Diptera or the Hymenoptera.

RELATION BETWEEN GAME AND THE TSETSE FLY

Throughout South Africa, according to Hamilton (25), there is a widespread belief that the "big game" mammals are responsible for the spread of the tsetse fly. Many have advocated the destruction of all big game in order to destroy this fly. The relation of the tsetse flies to sleeping sickness and to cattle and horses has aroused great interest in its habits. As a result last year saw the appearance of several articles (20, 24, 25, 38, 50, 55) treating of the habits and the habitats of the tsetses. To those interested in the economy of the species, all of these articles will be of value. Two of them (25, 50) produce conclusive evidence that the presence of "big game" is not essential for the existence of these flies, and that the destruction of the large mammals does not necessarily destroy the tsetse fly. The deduction is based on the following evidence. According to Sharpe (55), when the rinderpest destroyed much of the game in Nyassaland, there was no diminution of the number of tsetses. J. S. Hamilton (25) asserts that in certain regions hunters have almost destroyed the large game without causing any perceptible diminution in the number of tsetses. Both of these men assert that the fly often exists in great numbers in regions where there is no large game and is sometimes absent from regions where there is an abundance of game.

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LITERATURE FOR 1911 ON THE BEHAVIOR OF ANTS AND MYRMECOPHILES

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Bouvier (1) observed the moving habits of the harvesting ant, *Messor barbarus,* at Royon. This ant nests in very large colonies in subterranean nests, of which one colony may have more than a single one. When conditions in one nest are unfavorable, the ants move into another taking with them their young and the stores of grain. During the process of moving a preliminary body of ants go out. These travel in two columns from one nest entrance to the other. The transportation of the larvae, pupae and the food supply is carried by a double column also. The small crustacean, *Platyarthrus hoffmanseggi,* which lives in great numbers in the nest entrances of *barbarus,* follows the ants from one nest to another, probably finding the new abode of the ants by the scent trail which these have left.

Buckingham (2) made very extensive experiments on the division of labor among ants as correlated with size and structural differences. The ants used were two species of Camponotus (*C. americanus* and *C. herculeanus pictus*) and three of Pheidole (*Ph. pilifera. Ph. vinelandica* and *Ph. dentata*). Camponotus was chosen because of the presence of large and small workers connected by intermediate forms, and Pheidole because it has small workers and large soldiers, without intermediate forms.

A tabular arrangement of measurements of Camponotus shows that there is a perfect gradation between the largest and the smallest workers, both in size and in structure. Structurally the queen is most closely related to the major worker. In Pheidole, no such intermediate forms exist, and the queen similarly is more like the soldier, though resembling the worker in certain characteristics.

In working with ants in artificial nests it was found that the ants were stimulated by a rise of temperature rather than by light, remaining quiet at 22° in a good light, but moving about when the temperature was raised from 19° to 20° and 21° C. The stimulus was assumed to be not simply heat, but a rise in temperature.

Experiments were made with ants in Fielde, aluminum and Barth nests, and observations were made on outdoor nests. With the latter field notes were used, and ants engaged in various activities were collected and the data recorded. The activities tested were 1, foraging; 2, partaking of different foods; 3, feeding themselves; 4, regurgitating to others and receiving regurgitated food; 5, licking each other, likewise being licked; 6, tending the young; 7, building; 8, carrying other ants, likewise being carried; 9, surrounding the queen; 10, fighting; 11, responding to disturbances of the nest; 12, guarding; 13, scavengering.

An elaborate series of experiments was made, not only on the relation of the size to the labor, but also in regard to age. Callows, or recently hatched ants, acted chiefly as nurses, taking no part in the defence of the colony or in building. The size of the various workers was shown to be well correlated with the activities, most of the work in the nest and the foraging being done by the more active, smaller forms. The majors, both in Camponotus and Pheidole, were ordinarily sluggish, but easily stimulated by the nest's being disturbed, when they became more active than the minors in defending the nest. This is a valuable adaptation, keeping the majors in readiness and reserve for fighting. Buckingham shows that workers and even queens of various sizes participate to some extent in the activities of the colony, but a preponderant number of those working (with the exception of defending the nest, comparatively few of the ants in a colony are working at any one time) showed distinctly that the functions of the ants of different size are correlated with their structural differences.

Champlain (3) records the capture of *Xenodusa cava* in numbers in Connecticut, on tree tanglefoot which is used to trap the fall cankerworm. Ants also had fallen victims.

Cornetz (5, 6, 7, 8) and *Cornetz* and *Bohn* (11) in Algeria studied the homing of several hundreds of individual ants. Seven species were observed. An ant on going away from the nest strikes out in a definite direction, which it maintains in a general way throughout the out-going trip. From time to time in searching for food it deviates but always resumes the original direction. To an ant (Messor) at several meters distant from the nest, Cornetz gave a seed. It took this in its mandibles, turned about and returned directly to the nest. This experiment was repeated a great many times, always with the same result. The line of return was more direct than the outgoing line, which was rather sinuous, with frequent deviations. The axes of these curves were followed in returning, and these led, with slight inaccuracies, toward the nest. In the immediate vicinity of the nest the ant stopped and searched for the entrance, showing here a realization of having gone a certain distance. This sense is quite variable. Sometimes the ant stops altogether too soon. When the ant first experimented upon was away from the nest Cornetz changed the aspect of the returning line by sweeping, digging ditches and bringing piles of earth or rubbish. The ant returned without hesitation, along a nearly direct line toward the nest. This experiment, which was repeated many times, shows conclusively that sight, smell, touch, chemotropism or topochemism play absolutely no rôle in the return of the foraging ant to the nest, except in the immediate vicinity of the entrance, when these senses may be brought into play. As far as these experiments went they seemed to show the existence of some mysterious "homing" sense, but further experiments were made that utterly disproved any such supposition.

On a plantain leaf, left in the vicinity of a nest until the ants had become accustomed to its presence, Cornetz placed a number of seeds. An ant (a large Messor) climbed upon this and seized one of the seeds. Cornetz carried the leaf several meters away and placed it again on the ground. The ant was then absolutely incapable of returning directly to the nest, but began to wander about, increasing the extent of its searching, but not further on one side of the point than on the other, until it approached the immediate vicinity of the nest. This experiment, repeated 150 times, always gave the same result. When carried to a distance of two or three meters from the nest, it took from a half to three-quarters of an hour before the ant could find the nest entrance, which, had the ant walked such a distance from the nest, would have taken only a minute or two. From this it is evident that the ant possesses no sense of direction. The return after going out from the nest is explained by assuming that the sensorium of the ant receives a sensation of direction on the outward trip. This sensation is received at the beginning of the trip and maintained throughout its course, for no matter how many times the ant deviates from the general direction it returns each time. This phenomenon Cornetz calls "the constant reappearance of the once impressed wandering direction." This direction memory of the trail may be of greater complexity. When ants had become accustomed to a pile of grain at a certain point, the grain was removed some distance. The ants diverged from the old spot, found the grain and returned to the point at which they had left the outgoing trail, and then went directly to the nest. Here a memory of two separate directions is shown. The secondary trails, diverging from the principal outgoing trail, were at right angles to it.

Cornetz concludes from these observations that sight, smell, touch, chemotropism and topochemism play no rôle in the return of a solitary foraging or hunting ant, but that it is guided by an impression received by the sensorium at the beginning of the outward trip. From the fact that the outward trail is sinuous while the homeward is more direct, it is evident that the ant does not possess a kinometric muscular memory. Touch, smell and sight are probably valuable in guiding the ants in the immediate vicinity of the nest, where they lose the earlier impression of direction. But after a heavy rain, that had lasted two days and changed completely the general surroundings of the nest, the ants went out and returned as readily as before, when they had been familiar with the immediate vicinity of the opening.

Cornetz (9) reviews Santschi's paper "Observations et Remarques Critiques sur le Mecanisme de l'Orientation chez les Fourmis," in which the effect of the sun's rays was said to be an important factor in orientation. This phenomenon Cornetz calls the "solar tropism of Santschi." He considers that it helps greatly in explaining the constancy of orientation, but doubts its application to ants moving about in the deep woods, or in the shade in general, unless we assume that certain as yet unknown rays of the sun penetrate through walls, trees, etc., not being absorbed by these bodies. When the sun was very high, Santschi's experiment was not successful, and Cornetz points out that beneath the equator, with the sun more or less perpendicular to the line of march, the ants are able to maintain their direction.

Cornetz notes a case of a memory of direction in *Myrmecocystus bicolor,* which had moved from one nest to another, and for a long time traveled between the old and new nests.

Davis (12) records the occurrence near Clayton, Georgia, of a mixed colony of *Formica exsectoides* and *Formica subsericea,* representing one of the stages in the colony formation of the former species, which is a temporary social parasite.

Donisthorpe (13) placed the larvae of a Chrysomelid beetle (*Cryptocephalus fulvus* Greg.) in a plaster nest with a few of its host ant, *Lasius fuliginosus.* The larva acted like that of the related genus Clythra, crawling about and feeding on the refuse of the nest, and enlarging its case as it grew. When the adult beetle emerged it was torn to pieces, either by the ants or by other myrmecophiles (Myrmedoniae) which were present. In nature the mature beetle would escape from the nest.

Donisthorpe (17) placed workers, eggs and larvae of *Formica exsecta* Nyl. into a small nest of this species which the year before had been taken near the same spot. The workers intermingled freely and behaved in a friendly manner towards one another. He records *Lasius umbratus* as collecting small empty land shells, and sitting on them. No explanation is offered of this curious behavior.

Two colonies of *Leptothorax acervorum* were placed together, and apparently affiliated to form one colony. Donisthorpe has never reared a female from eggs laid in captivity, and suggests that the juices obtained from aphids may be necessary to the development of the female larva.

The myrmecophilous beetle *Atemeles emarginatus* Pk. was taken from a nest of its winter host *Myrmica scabrinodis,* and placed in a nest of *Formica fusca.* The beetles remained away from the ants for twenty-four hours, when, joining the ants, they were readily received. This is regarded as additional evidence that these beetles go into quarantine when leaving the nest of one species of ant before entering that of another.

A spider, *Asagena phalerata,* was observed catching ants (*Myrmica laevinodis*). It kept at some distance from the ant and "threw the threads" of web at it. Four or five ants were entangled together and carried off. The mite *Laelaps cuneifer* Mich. was observed to follow the "tracks" of the host ant when it went from one nest compartment to another.

Donisthorpe (15) records observations on the behavior of certain ants that are temporary social parasites. Experiments were made with *Formica sanguinea* Latr., the only English slave-making ant, and its host, *Formica fusca.* Workers and pupae were placed in one compartment of a "Crawley" .nest, and two females of *sanguinea* in another. As soon as the latter became aware of the presence of the *fusca* cocoons, "they hurried in, attacked and killed the *fusca* workers and carried the cocoons to their own compartment." The adults from *fusca* cocoons which were placed in the nest, were assisted in emerging by the *sanguinea* queen.

To prove that *sanguinea* females are unable to found colonies by themselves, Donisthorpe placed several in bowls. They hid beneath pieces of sponges, but made no attempt to dig cells. A few eggs were laid, but neglected, and therefore did not hatch, thus corroborating other experiments and seeming to indicate positively that *sanguinea* depends absolutely upon a temporary host to found a colony.

Emerton (18) states that the spider *Phrurolithus formica* Banks is found only in the nests of *Cremastogaster lineolata.* In the spring, when the ants are gathered under stones over their burrows, the spiders are found among them. If not frightened, they move about slowly, like the ants, but when frightened are much more active, and run either into the ant burrows or into the grass about the nest.

Emery (20, 21) studied the mating and colony-founding habits of Polyergus. There is no doubt that marriage-flights occur frequently with Polyergus, but copulation may also take place in the nest. Queens, both deälated and winged, have been frequently observed in company with raiding-columns. In one colony observed by Emery for two years, the marriage-flights occurred the second year, but not during the first. The first year, queens were found with raiding-armies. Of these Emery sent one deälated specimen to Silvestri, who found the *receptaculum seminis* full of sperm. A winged female taken at the same time was not fertilized. No queens were found in the columns of workers outside the nest during the second year, and Emery considers it possible that marriage-flights may depend on the number of sexual forms, especially of males. Where there are only a few of these copulation takes place in the nest.

In founding a colony, one or more Polyergus queens enter the nest of *Formica fusca* or some variety of that species. When not prevented by the workers the parasitic queen goes directly to the *fusca* queen and kills it by piercing its brain with the sharp mandibles. The demoralized *fusca* workers then readily adopt the alien queen. Some of the Polyergus were killed by *fusca* workers before reaching the queen. This was in a Janet nest, which is particularly favorable to the Polyergus queen. In nature the killing of parasitic queens probably occurs more often.

It is important to Polyergus that the host colony be populous. This Emery believes to be true of all parasitic ants. If the colony selected by the queen is small, the primitive stage of the Polyergus colony may last two years. During the first year no eggs are deposited. The second year eggs are laid and workers develop, but no raids are started until late in the summer, sometimes not until the third year, when the colony is fully developed.

Emery (19) studied the relations of fecundated females of *Pheidole pallidula* toward each other. They oviposit a few days after fertilization, and when placed together in an artificial nest fight and tear the antennae and legs of one another. The individuals without antennae become indifferent to the eggs, so that their care falls to the lot of the female that has preserved her appendages. Eggs laid early in July gave rise to workers August 15th. After three separated females had been permitted to bring up their broods, Emery made openings between their cells. The workers at first tried to obstruct the openings. The workers of two of the colonies fought with one another. When the queen of one of these was killed her brood was appropriated by the other. Later the two remaining colonies behaved in the same manner till only one female remained and the broods of all three colonies were united. When several females were permitted to found their colonies in close proximity to one another, similar results were obtained. Emery concludes that recently fecundated queens of *Ph. pallidula* live together amicably till they have oviposited, but that discord supervenes with maternity and the attraction of the ova.

Experiments with deälated queens of Tetramorium yielded somewhat similar results, but out of fifteen queens that reared their broods together, three survived. In other species of ants the queens are even more tolerant of one another, e.g., *Plagiolepis pygmaea* and *Leptothorax unifasciatus*. In this connection Emery calls attention to the Argentine ant (*Iridomyrmex humilis*), the colonies of which, according to Newell, may unite in great numbers in the fall, producing enormous hibernating colonies, each containing hundreds of queens.

Emery (22) shows that larvae which are insufficiently nourished produce weak and undersized workers. These inferior workers are found in the first brood of a queen and in colonies where the worker force is small numerically. As the success of a colony of robber-ants depends on the presence of large, strong workers for fighting, Emery considers that a populous host colony is necessary. Social parasitism having arisen, in the opinion of Emery, from nest-robbing habits, this necessity for a large force of workers to attend the young has been an important factor in its origin.

There is no transition in habit from the forms of *fusca*, through to the typical social parasites, *rufa*, *truncicola*, etc., so Emery considers that the social-parasitic habit originated from a mutation rather than by a gradual development. In *sanguinea* the mandibles of the male are toothed, while in *fusca* they are smooth, which shows that the latter species is even more specialized than the former.

Enslin (23) observed *Formica cinerea* attending the small European Membracid, *Gargara genistae*, which he found in numbers on *Sarothamnus scoparius*. The plant was frequented by ants which were foraging and also attending a few aphids in the ordinary manner. When one of the ants finds a Gargara nymph it strokes the abdomen with its antennae. The treehopper elevates the abdomen and from the anus exserts a rod-like structure on the end of which is a drop of light-colored liquid which the ant imbibes. Then the ant either tries again on the same individual or hunts for another. If the head or thorax instead of the abdomen of the bug is stroked the result is the same. Adults of Gargara are also attended, but not so frequently as the immature stages, possibly because the drop is harder to get at on account of the wings. One adult was stroked for some time by an ant and failed to respond, but a half minute later when the ant had gone it spontaneously ejected a large drop, which was found and lapped up by another ant.

Enslin placed specimens of both the immature and mature phases of the tree-hopper in the immediate vicinity of a *cinerea* nest. Sometimes an ant received a drop of secretion from the Homopteron and went on its way. Others took the insect in their mandibles and attempted to take it into the nest, sometimes successfully. At other times the insect struggled too much. The fate of those taken into the nest was not observed. Without access to its food plant the insect could not secrete the liquid for which the ants attend them; besides, those which Enslin kept without food died within two days. It may be that those adults brought in after copulation deposit eggs in the nest, but nothing definite regarding this is known. In fact, only scant observations on the relations between any Membracids and ants have been recorded.

Escherich (24) in Ceylon studied the "myrmecophilous" plant Humboldtia, the stems of which have hollow internodes, each with an opening at the upper end. The leaves are provided with nectaries that afford a food supply to ants, several species of which nest in the hollows, and Schimper had considered the plant typically myrmecophilous. Escherich found that only a small percentage of the internodes were used as nesting sites by ants, and that these were species which nest in various other places also. Unless the twigs were violently shaken the ants made no attempt to defend the plant. Many of the occupied stems showed scars made by woodpeckers attracted to the ants, so Escherich concludes that the relationship between plant and ant is in no wise symbiotic, but rather parasitic, as the latter derives its shelter and food from the former, giving no service in return, and even being directly harmful through attracting enemies. Escherich believes that records of symbiosis based on the study of the plant structure only, without observations on actual relationships, are not worthy of consideration.

In Abyssinia Escherich observed the foraging habits of a species of Messor. During the day the ants remained in the nest. At nightfall they emerged, leaving the nest by a distinct trail which was followed 30 or 40 meters. Then they spread out, some climbing weeds in search of seeds. When food was found the ants returned by the same trail, which was then being traversed by two files of ants moving in opposite directions. The seeds gathered were of numerous varieties. Some of the foragers returned carrying lumps of earth, small stones or other worthless objects, a fact tending to show variability in the grain-collecting instinct. At the nest entrance several large workers acted as guards and examined with their antennae the returning workers before allowing them to enter the nest. The husks from seeds were brought out of the nest very quickly and taken to some distance.

Among the foods collected by the ants were many tubers from the root stalks of *Cyperus bulbosus,* which are eaten also by the Abyssinians. Their carrying these about may possibly be of importance in the dissemination of the plant species.

Goeldi (25), in an account of the structure and character of the ant nest, introduces the term Gonepitropy for the assumption of the reproductive functions by the queen. The workers have taken over the functions of food-gathering and nursing for the entire colony, and this Goeldi calls Ergepitropy. The workers Goeldi does not consider to be degenerate females, but rather a distinct caste, fitted for the work that is done by them.

Krausse (26) records a few observations made on *Aphaenogaster sardoa,* a characteristic Mediterranean ant. The holes in which this ant lives are not dug by it, but have been made previously by other animals or natural means. In the fall the ants cluster into a ball, holding on to each other with mandibles and tarsi, with the eggs, larvae and pupae in the center, and remain in a semi-dormant condition. This habit is analogous to the hibernation of northern ants. In summer the clustering habit to some extent persists, but the ants are much more active. Krausse placed a number of workers with pupae in a glass nest and after a half hour found them in a compact cluster, the pupae in the middle. When dispersed, they again built a cluster. No myrmecophiles were found with *sardoa,* and Krausse considers their absence related to the non-harvesting and non-cell-building habits of the ant. The larva-robbing Dermapteron, *Euborellia moesta,* which steals from the nests of other ants in the same vicinity, was not found with Aphaenogaster, probably because of the larvae being in the center of the ant cluster where they can be taken only by a direct attack, a method to which few inquilinous thieves resort.

Krausse lists a number of ants which are known to possess stridulating apparatus. In some of the species the sound produced is loud enough to be heard by the human ear.

Krausse (27) in Sardinia found carton nests built beneath stones by *Cremastogaster scutellaris.* Besides *C. scutellaris, Lasius fuliginosus* and *Liometopum microcephalum* are European ants which make carton nests.

With *C. scutellaris* myrmecophilous insects were rare. One specimen of the robber earwig, *Euborellia moesta,* was found in the nest of this ant.

Krausse (29) observed two ant trails which crossed each other. The trails were made by different species of Messor, and led from nests which were very close together. Ants in the files going and coming showed sometimes a little excitement at the point of crossing, but no actual fighting took place. They would lunge at each other without taking hold.

Krausse (28) records an earwig, *Euborellia moesta* as occurring commonly in the nests of several ant species in Sardinia. Its relation to the ants is that of a robber, and it destroys large numbers of the larvae. *Forficula auricularia* was also observed to enter ant-nests and feed on the immature stages.

Leonard (30) records observations made on "honey" ants, *Myrmecocystus mexicanus mojave, M. mexicanus* and *M. melliger lomaënsis,* all found on the grounds of the Raja Yoga School at Point Loma, Cal. *M. mojave* prefers a moist soil in which to nest. Leonard believes that the large entrances to the nests are required for the removal of iron nodules encountered in digging, and not for purposes of ventilation as suggested by Wheeler. This new view presupposes large quantities of iron nodules in all regions inhabited by *mojave,* for the habit of making large entrances is characteristic of the insect throughout its geographical range.

Individuality in character was shown by a minor worker. When one of the ants had fallen into a moat two workers discovered and inspected it without offering aid. Then the minor approached and "showed the liveliest concern. For many minutes she vigorously kneaded the patient's gaster, and worked the stiff legs until at last the half-drowned ant revived." The ants gather honey from blossoms of a pepper tree (*Schinus molle*) and from *Euphorbia setiloba*, *Echimus simplex*, *Ceanothus cuneatus*, and from aphids.

Callows, after emerging, were carried out of the nest by older workers "as a hint that they might now undertake the regular work of the nest."

Myrmecocystus mexicanus was observed to lap the honey contents of dead repletes, contrary to the recorded observations of McCook on the var. *horti-deorum*. *Myrmecocystus lomaënsis*, a diurnal species, became violently excited when it was offered bee's honey.

Miehe (32) studied the plant Myrmecodia in Java in relation to ants. *Myrmecodia tuberosa* possesses a swelling in the stem, originating from the hypocotyl. This swelling is perforated by a complicated system of cavities. One or more large openings connect these with the surface, and smaller openings are also present. In the inner spaces of the cavities numerous species of ants take up their abode. These sally out when the host plant is struck, but are not particularly aggressive.

Forbes and Treub found that these cavities were developed without the presence of the ant, in contradiction to earlier opinions. Treub considers that they are useful to the plant in transpiration. Kaesten regarded the swelling as an organ for the economy of water, the wart-like objects (*lenticels*) which line the cavities being absorbent organs, and Miehe also considers them to have the same function. They are also supposed to excrete water.

One part of the cavity walls is light in color, smooth and without tubercles, the other is tuberculate and dark in color. On the latter surface flourishes a fungus, which in the opinion of Miehe grows on the excrement of the ants. The ants have their larvae and pupae always on the smooth surface, and deposit their excrement on the rough part, a habit similar to that recorded by Von Ihering for the Azteca queens of the South American Cecropias. According to Miehe the food products in the ant excrement must be important to the epiphyte.

The fact that the ants drop their excreta on the exact place where the absorbing organs are situated, and that this is of importance to the plant, is taken as evidence of myrmecophily, but whether the dung is actually necessary to the growth of the plant is not known. The plant has few natural enemies, so its defense by the ant is not to be considered. *Iridomyrmex myrmecodiae* lives in other places as well as in the hollows of Myrmecodia, so that the plant is certainly not necessary to the ant. *Hydnophytum montanum* is recorded as having similar relations to the ants.

Nickels (33) experimented as to the possibility of exterminating the Argentine ant (*Iridomyrmex humilis*) with entirely satisfactory results. Sponges, moistened with sugar syrup, containing one-fourth to one-eighth per cent. sodium arsenate, were placed in the neighborhood of the nests. This was eaten by the ants, and fed to the queen and brood, slowly poisoning them and shutting off the increase of the colony.

Polimanti (34) in Naples observed large numbers of the winged forms of *Lasius niger* attracted to large arc-lights, while comparatively few came to incandescent and other less bright lights. He attributes this to simple phototropism.

Poulton (35) published some notes made by Lamborn, who had noticed that the larvae of Lycaenidae in the Lagos district were often attended by ants. Examination of the ant-nests in the hollowed head of a plant (*Castus afer*) showed that each contained several caterpillars and pupae. These were attended and licked by the ants, but the adult butterfly was killed and eaten. Lamborn observed the ant *Oecophylla smaragdina* attacking the larva of a Lycaenid. This ant has been observed frequently to tend the caterpillars. The larva of a Pyralid moth also lives in the nest.

Reichensperger (36) confirms the observations of Wasmann that in nests of *Formica sanguinea* infected with the beetle Lomechusa, normal queens and pseudogynes are not developed at the same time. The pseudogynes which he found were intermediate between those forms which Wasmann calls Mikro- and Mesopseudogynes. The percentage of these abortive females in the brood gradually increased, the slave-making raids were not continued, and those pupae already taken were not cared for, the result being the weakening and the final disintegration of the ant colony, as a result of the presence of Lomechusa.

Reichensperger observed for the first time the first stage in the temporary social parasitism of *Formica pratensis* in the nest of *Formica rufibarbis* var. *fuscorufibarbis*. The alien queen was very readily adopted, "treated as though she were the only one," one beginning to lay eggs after one week. *Pratensis* queens are not always so successful, as was shown by the finding of dead and dismembered individuals in other nests of *rufibarbis*.

Reichensperger records the discovery of a microgyne of *Plagiolepis pygmaea*. One of these small queens in an artificial nest laid eggs from which normal workers were reared. The microgynes were similar to the queen in structure, but in size about as large as the workers, from which they differed in color as well as structure.

In a postscript Reichensperger states that he may not have been correct in his specific determination. The microgyne may have been a guest species, or an incipient new species derived from *P. pygmaea*.

Sadownickowa (37) made a series of 42 stereoscopic photographs of various activities of European ants. Among the lot are pictures of the nests of several species, life histories, foraging individuals, parasitic colonies, fighting, and mixed colonies. Photographs No. 40 and 41 show ants of different species, from which the antennae have been removed, living peaceably together. In the lot are workers of *Camponotus ligniperdus*, *Formica sanguinea*, *Formica rufa*, *Formica fusca*, *Messor barbarus*, a female of *Formica sanguinea* and a male of *Lasius niger*, all feeding on honey water. The antennae of the male have not been removed, but it is apparently unable to distinguish between friend and foe.

Santschi (38) studied the behavior of several species of North African ants in regard to orientation. Certain of these (*Acantholepis frauenfeldi* and *Tapinoma nigerrimum*) made a scent trail by touching the tip of the abdomen to the ground. *Myrmecocystus bicolor*, with the antennae removed, found the nest, at first wavering somewhat but more directly as it neared the nest, evidently familiar with the immediate approach. Here sight seems to be an important factor. *Messor barbarus* proceeded in the general direction of its nest, though the ground over which it had come had been swept and otherwise disturbed, so as to leave no possibility of guidance by scent or the sight of immediate objects. The sight of this ant is poor so that objects at a considerable distance from the ant could have had no effect, so the experiments agree with those of Cornetz and other workers in showing the constancy of conservation of orientation. Santschi explains this by assuming that on the outward-going trail certain rays of light (probably ultra-violet) make an impression on the pigment of the ant's eye. Owing to the structure of this organ these impressions are unequal in various parts and are retained, making a sort of compass to orient the ant on its return. By means of a mirror, Santschi changed the direction of the light rays on homeward bound ants. Under this influence the ant changed its course, but orientated itself toward the nest when the mirror was taken away. Santschi considers the "homing" sense a very complicated phenomenon, involving many factors; but above all, the perception of these physical rays, visible or not to us, is valuable as a basis for orientation.

Tanquary (39), in order to determine whether they would be adopted, placed queens of *Aphaenogaster tennesseënsis* with colonies of *A. fulva, A. fulva aquia, A. fulva aquia* var. *picea*. In all cases the queen was eventually killed by the workers of the other species, but a tendency toward adoption was shown by the fact that the queen was often licked by the workers. A queen of *A. tennesseënsis* was placed with pupae of *A. aquia*. She paid no attention to these. Some hatched, and at first the queen remained by herself, but afterwards stayed with them, and was readily adopted, as in the cases recorded by Miss Fielde (Artificial Mixed Nests of Ants).

Queens of *Formica rufa* subsp. *obscuriventris* were placed with colonies of *Formica fusca subsericea*. Where the colony was large, the queen was killed; when small, after being attacked for a time, she was finally adopted. The queens showed decidedly inquilinous habits, and this, with the fact of their being adopted by small colonies, with the previous finding of a mixed colony in nature, shows that *F. obscuriventris* is, at least occasionally, a temporary social parasite in the nests of *F. subsericea*.

Experiments were made with 79 queens of *Lasius* (*Acanthomyops*) *latipes,* introducing them into different colonies of other species of Lasius. Of all these, only two queens were completely adopted. This low percentage can be partially explained by the difficulty of keeping queens of *L. latipes* alive in confinement. It is doubtful if in nature even this small proportion of the queens succeed in forming colonies, for colonies of the species are not very abundant, in spite of the fact that a great number of queens are produced in each colony.

Queens of this species and of *Lasius americanus* were placed together. Neither species showed hostility toward the other, though their temperaments were decidedly different, *latipes* being very restless, and running constantly about. The fact that both species of queens lived amicably together might be taken to indicate that mixed colonies may be founded in common by two queens of different species, but the nervous behavior of *latipes* and its attraction to any brood, with which it settles contentedly, is taken by Tanquary to show that it is more plausible to believe that this species is a temporary parasite.

Experiments were made with 88 queens of *Lasius umbratus* var. *minutus* and workers of several other Lasius species. Of this lot only one queen was adopted by *L. americanus*. But this one case indicates that *minutus* is parasitic on *americanus*. The *minutus* queens are feeble, and easily killed, succumbing to treatment that would not be fatal to ordinary species. The sporadic occurrence of the species, with the large number of queens produced (a quart of earth from the one nest was found to contain about 150), is taken as justification of this conclusion.

Queens of *Polyergus lucidus* were placed with colonies of *Formica incerta* and *F. schaufussi*. The queens were attacked by the workers, but defended themselves with vigor, one of them killing all of the fifteen workers with which she was confined. In two experiments the queen was adopted by *incerta* after being at first attacked. An *incerta* queen in one of the nests was killed by Polyergus, after which the workers adopted the alien queen. In no case did any of the Polyergus queens pay any attention to the young.

Queens of *F. nepticula* were tried with colonies of *F. incerta*, *F. fusca* var. *subaenescens* and *F. neogagates*. With the first two the results were negative, the queen being killed very soon, but with *neogagates* the attacks were not so fierce. Several times workers were observed licking her, and she was twice observed feeding a worker. She was finally killed.

Viehmeyer (40) studied the mating flights in several species of ants. These flights have a double purpose, to distribute the species and to prevent close inbreeding. The flights of the same species occur at approximately the same time, and at these times sexual forms from many nests mingle together in an enormous swarm, so that the individuals from one nest copulate with those from another. The wings of queen ants are very brittle and weak, and the presence of a strong wind blows the ants about, scattering the species to long distances. Often many species of ants have their mating flight at the same time, and join in one common swarm. One of these swarms was observed by Viehmeyer. It consisted of two species, *Myrmica scabrinodis* and *M. ruginodis,* with the latter greatly predominating in numbers. When numbers of pairs had fallen to the ground they were collected. Of the lot, only about one-sixth were of the same species, and in every case these were *ruginodis.* Most of the couples consisted of a *scabrinodis* queen and a *ruginodis* male. The males of both species predominated in numbers over the females. Hybrids between ants are sterile, and Viehmeyer does not consider that hybridization has any importance in the phylogenetic history of the species.

Wasmann (42) treats, in a very general way, ants and their guests, dealing especially with social parasitism of the former, and the relations of the latter to their hosts. Some two thousand species of Myrmecophiles have been recorded. These Wasmann has separated, according to their relations to the host, into five classes, (1) Symphiles, or true guests; (2) Synoeketes, or indifferently tolerated guests; (3) Synechthrans, or persecuted guests; (4) Ecto- and endoparasites, and (5) Trophobiotic symphiles. A short account of the Symphiles is given. With the legionary, or driver ants are three types of guests, called by Wasmann symphilic, mimetic and protected ("Trutz") types. The latter are tolerated by the ants because they are unable to destroy them. This paper is a very excellent general account of the subject, and is illustrated with several photographic plates.

Wasmann (44) shows that in species of Atemeles living with different hosts, varieties have originated which have characters correlated with certain characters of the host. These are differences in size, color, sculpture and especially in abundance of hairs. Such forms, he believes, should be considered as varieties or subspecies and not as aberrations, as has been suggested by other writers. These variations adapt the beetle only to its summer host (Formica), each species of which has its particular form of guest, while with Myrmica, the winter host, several varieties of Atemeles may live.

Wasmann (46) describes a new Paussus from Ceylon, and gives a list of the host ants of the family Paussidae, so far as these are known. The hosts of only a small number (about one-sixth) have been recorded. The Myrmecine ants, especially of the genus Pheidole, most commonly harbor the beetles, but several live with Camponotinae, and one species, *Arthropterus brevis* West., lives with *Ectatomma metallicum,* an Australian Ponerine ant. None have been taken in nests of the Dolichoderinae or with the Dorylinae. The Paussidae are the oldest known ant guests, occurring in the Baltic amber. These amber-imbedded forms are similar to living species, and the branching of this family from the Carabidae must have taken place at a very early date. Wasmann doubts whether the structure of the beetles is the result of symphilic life, and points out the fact that Pheidologeton is known from the upper Oligocene, while the characteristic host genus, Pheidole, occurs first in the lower Miocene.

Wheeler (47) compares the ant colony to an organism, its behavior and structure with a gigantic foraminiferous Rhizopod. The nest, queen, mass of workers and files of foragers represent respectively the shell, nucleus, plasmodium and pseudopodia. The ontogenetic development of the colony is clearly comparable to that of a complete organism. The fecundated queen represents a mobile, fertilized egg, capable of disseminating the species, and also of selecting the site for the development of the colonial organism. The mass of workers make up the nutritive organ. The first of these are nourished with food derived by metabolism from the fat body of the queen, and from her useless wing muscles. The first young soon begin to forage, to build the nest and to care for succeeding broods. When sexual forms are developed the organism may be considered to have reached maturity. The phylogenetic history is also similar to that of an organism. A complete gradation exists from the solitary female Hymenopteron, through the small colonies of primitive monomorphic workers closely resembling the female, to the large complex colonies of highly specialized, polymorphic ants, conforming in a most striking manner to the biogenetic law.

The phenomenon of budding is represented in certain ant colonies by the production of numerous queens, some of which, each accompanied by a band of workers, separate from the parent colony and found another. Alecithal eggs are comparable to the diminutive queens of parasitic ants, which, incapable by themselves of founding colonies, enter nests of alien species and use the workers as the nutritive organ, until their own young are developed. These temporary and other forms of social parasites are directly comparable to entoparasitic organisms. Ectoparasitism is represented by compound nests of two or more species which live side by side, sometimes mingling freely, but keeping their broods separate. The restitutional tendency of organisms is found in the replacing of destroyed workers by the rearing of more, and in the development by certain workers of ovaries, when the queen is removed.

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LITERATURE FOR 1911 ON THE BEHAVIOR OF VERTEBRATES

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VISION

Fish. C. Hess (13) offers a new piece of apparatus for the study of adaptation in fish. At one end of a dead black tunnel three metres in length he places a five candle power electric light with frosted bulb. At the opposite end of the tunnel and inclined at an angle of 45 degrees to its axis he places a diffusing surface of ground glass. The light from this surface is reflected out to a dish which contains the fish. Large changes in intensity are made by substituting lamps of higher or lower candle power, while fine gradations are obtained by the use of the rotating sector and by moving the lamp. The animals are light-adapted in the sun and then quickly placed in the experimental tank. The animals respond by swimming towards the side of the tank receiving the rays from the ground glass as soon as the process of adaptation is sufficiently advanced. For example, the carp exposed to the rays of the sun for one-quarter of an hour, show in the first second of time no tendency to swim towards the light when the 5 c.p. light is 30 cm. from the ground glass. After 20-30 seconds in the dark room, they swim toward the lighted side when the lamp is moved back 1-2 metres. After 10-15 minutes adaptation, they swim towards the light when the lamp is 3 metres distant from the screen and the rotating sector set with a 30 degree opening. He concludes that light sensitivity is increased over a thousand fold after fifteen minutes of adaptation. In some cases even a greater increase in sensitivity is found.

He finds an essential difference between the human and the fish eye. In man the retinal pigment even in long exposure to light does not essentially change its position, while in the case of the fish the pigment upon light exposure wanders soon toward the vitreous humor. This may offer a physical explanation for a small part of the process of adaptation, but Hess maintains that the process as a whole is physiological.

The forward migration of the pigment in light exposure alters the general effect of the light. The advanced pigment absorbs relatively more of the short wave-lengths than of the long. In order to obtain a relation of equality (for the fish) between given red and blue lights one must use a blue 4-8 times as intense for the eye with pigment advanced as that used for the eye with the pigment retracted. The pigment absorbs little of the green rays. This is shown by the fact that the fish which collect in the brightest part of the container seek the yellow-green both when the pigment is in the forward position and in the retracted.

The physiological changes in light (monochromatic) intensity incident upon the migration of the pigment might easily be confused with the Purkinje phenomenon, as was done by Bauer. Hess still emphatically denies that there is any such phenomenon present, and reaffirms his old conclusion that the fish are color blind. It is interesting to note that he everywhere emphasizes the low stimulating value of red.

Bauer (1) in an extended reply to Hess (see this journal. Vol. 1, p. 432, for review of Hess' criticism) reasserts his conclusions that the fish in the light-adapted state are strongly stimulated and even terrified by red (rotscheu) and that they behave as though the Purkinje phenomenon were present. In the last paper just reviewed Hess (p. 419) says that Bauer's results are due to error, and that on repeating the experiments upon Atherina he finds that the animals *do not avoid the red either in the light-adapted state nor in the dark-adapted state.* If this is true, and Hess' work is everywhere consistent, the bottom of the quarrel drops out. Bauer's contention that the fish when light-adapted are normal in color vision and when dark-adapted are color-blind is absurd upon its face. His only real support of so strange a position lies in the asserted fact that the light adapted fish are "red-shy"—*i.e.*, that in addition to its brightness value red has a peculiar shock effect. Furthermore, from Hess' work on pigment migration we might very well expect a difference between the light-adapted and the dark-adapted fish in their responses to red and blue-green. This it would seem does away effectually with Bauer's claim for a Purkinje effect. The point is more important than would appear at first sight.

The Purkinje effect is absent in the totally color-blind, and if the fish are totally color-blind as Hess maintains, one could not explain a true Purkinje effect.

Reptiles and Amphibians. Experiments by Casteel (5) on the turtle (*Chrysemys marginata*) bring out the fact that the animals failed to discriminate between patterns. Four animals learned to discriminate between black and white, while one failed to show improvement in 220 trials. Two learned to discriminate vertical and horizontal lines and two between two series of parallel horizontal lines 8 mm. and 2 mm. in width respectively. One turtle learned to discriminate between two series of parallel vertical lines 8 mm. and 1 mm. wide and then between lines 4 mm. and 2 mm. wide and finally between lines 3 mm. and 2 mm. wide. Although not carefully tested, the retention of such habits was surprisingly good.

Casteel states that the turtle does not learn rapidly. One hundred and eighty-three trials were the average necessary to complete the above habits. From extended experiments (Hopkins) in which monkeys, dogs, rabbits and rats have been used as subjects in discrimination experiments it would appear that the turtle learns rather rapidly. It is not at all unusual for the above animals to require 200-600 trials to perfect similar discriminations.

Victor Bauer (2) defends the view that the visual purple functions in daylight vision as well as in twilight vision. His experiments were carried out upon frogs and rabbits. The retinae of these animals were exposed to light for varying periods under varying conditions, and the color of the retina was then determined. From his results he finds support for the following general conclusions. When the visual purple is bleached by the action of light, certain products are split off. Some of these products enter into reaction with the nerve substances and stimulate them, while others act upon the secretory epithelium surrounding the retina. The secretions resulting repair or reconstruct the bleached-out purple. The waste and repair of the visual purple takes place not only in dim twilight but even in strong daylight. This may be shown in the isolated bulb, in which the "Restitutionskraft" of the secretory epithelium is gradually exhausted by removal from the body. If sudden strong illumination is admitted to the eye which has been previously kept in the dark there is a corresponding rich formation of the bleaching products. The building up process is at first not able to repair the waste. Adaptation follows, and the building up process not only compensates for the waste, but continues until the retina which was bleached under the influence of the strong light again becomes red. There is an upper limit to the speed of the restitution process. If the decomposition is increased overwhelmingly, even the greatest activity on the part of the epithelium fails to restore equilibrium, and the retina remains bleached out. Only a decrease in illumination can restore the purple. After a more complete adaptation the "purpling" processes continue, but with lessened activity. In continued darkness the regeneration processes cease, probably in consequence of the lack of the by-products necessary for their arousal.

Birds. Hess (13) (p. 443) confirms by a new method his former conclusion that light-adapted day birds have an extension in the red end of the spectrum coincident with man's, but that the blue-greens, blues and violets have low stimulating value. The explanation of this is to be found in the reddish-yellow oil globules which are to be found in the retina of birds. These absorb the short but admit the long rays. It will be remembered that he in his earlier paper maintained the view that the hen has visual qualities similar to those of the human being, but that due to the presence of the oil globules there are certain differences in behavior. All these differences between hen, and man with normal vision, are smaller than between the hen and the totally color-blind or red-green blind man. I quote his own words (p. 445) on the coloring of the rice kernels for tests on the color blindness of the hen. I am in some doubt as to his meaning, or if I get his meaning, of the possibility of obtaining any such color equations: "Um Hühner nach dem Prinzip der Seebeck-Holmgren'schen Wollprobe mit farbigem Futter zu untersuchen, färbte ich grössere Mengen von Reiskornern mit verschiedenen Pigmenten so, dass mir insbesondere gelblichrote, angenähert rein rote und bläulichrote, sowie gelblichgrüne, angenähert rein grüne und bläulichgrüne Körner teils in schon freien, teils in mehr oder weniger stark mit Weiss, Grau oder Schwarz verhüllten Farben, ferner gelblichgraue, rein graue und bläulichgraue Körner reichlich zur Verfügung standen." (P. 445.)

Upon a dead-black table cloth 15 by 39 cm. in diameter he lightly fastened yellow-red grains and between these strewed the different greens and greys. On the second day the hen avoided not only the yellow-red grains, but also the red grains and the bluish red, and pecked only at the greys and greens. This was also the case when he strewed red grains among the greens and the greys (although in this case the red grains were no longer fastened down). With astounding quickness and sureness the animal pecked all the green and grey grains lying between the red grains. If there were only a few of these at hand, it hunted all over the entire cloth between the red grains so that after several seconds only the red ones remained untouched. If he brought such a hen into the dark room to a row of white rice grains, strewed upon a black background, which were not fastened down, and colored these by means of the spectrum, she here likewise left the red and yellow-red grains untouched.

He now laid before a so-called red-green blind person (relatively yellow sighted red-green blind) the differently colored yellow-red and blue-red rice grains and let him choose out of the pile the grains which seemed alike to him. This subject put yellow-green with yellow-red, pale blue with bluish-red. The differently colored grains which the hen had differentiated with such sureness were similar or alike to the red-green blind subject.

A hen which he had taught to avoid the red grains was fed on a black cloth upon which the green grains were fastened down while the red, etc., were scattered loosely. After a short time she no longer took the green grains when these were loosely strewn among the others, but she did take the red ones. In the spectrum she picked the grains lying in the red end as far as they were visible to the normal human eye.

Only for one with the visual qualities of the normal human eye have the different red grains the common characteristic of prevailing redness. He concludes that the possibility of a red-green blindness in the hens experimented upon is excluded, and greater strength is given to his contention that their visual qualities in this respect are similar to or the same as a normal man's.

Breed's (4) paper is concerned more with the modifiability of reactions than with the function of vision as such. Nevertheless experiments were made upon the color, form and size perception of the chick. The evidence for color vision, while obtained incidentally, is believed by the author to support the view that color quality as well as intensity is a determining factor in the chick's response. The chicks seem to be able to respond selectively to one of two objects of different size. The form tests gave purely negative results. In a note on page 57 the author states, however, that a chick acquired the circle-square habit when the experiments were conducted in the dark room. One interesting methodological point is brought out in Breed's work. Chicks will peck in the dark. The work of Hess and of Katz and Révész on vision in the chick is based upon the assumption that the chick will not peck at what it cannot see. The work which the reviewer has carried out upon the limits of the spectrum in chicks this summer wholly confirms Breed's findings in this respect.

Mammals. Minkowski (17) reports in a long and closely articulated paper the results of extended tests upon the localization of the visual centers in the dog. The methods of testing the visual field in normal dogs and of locating the visual defects in the dogs after cortical operation are described as follows. The dog sat with one eye securely bandaged. A piece of meat was then held by a slender rod or with forceps before the other eye at a distance of about 10 cm. This gave a reasonably steady fixation. A perimeter was then drawn forward in such a position that the bit of food occupied its center. A second bit of food was then drawn along one arm of the perimeter until the dog noticed the food and snapped at it. By rotating the perimeter in the usual way the following field was mapped out:

Upper vertical	60°-65°
Lower vertical	50°-55°
Outer horizontal	80°-90°
Inner horizontal	30°-35°

The "Kreisführungsmethode" was employed in a supplementary way. This consisted in moving the bit of food before both eyes of the dog in circles so that the image had to fall at some time upon the upper, lower, inner and outer portions of the retina.

The normal dog follows the food in all directions with pronounced movements of the eyes and muzzle. But where there are marked deficiencies in vision, as in the lower half of the *campus oculi sinistri*, the definite following movements suddenly cease, but begin again when the food is brought within the visual field. At the moment of disappearance, seeking movements of the eyes and muzzle may be noticed. This method gives quick orientation where the deficiencies brought about by cortical extirpation are great. The third auxiliary method he calls the "Auseinanderführungsmethode." Two bits of food are first held above the dog's nose and are then drawn apart in all directions; sometimes both are drawn apart simultaneously, while at others one remains fixed and the other is moved away. (At least so I understand "—dass zwei Nahrungsstücke zuerst über der Nase gehalten und dann verschiedenen Richtungen einzeln oder gleichzeitig auseinander geführt werden").

For the results of the experiments on extirpation of the cortical areas and for the anatomical findings the original paper must be consulted.

AUDITION

Fish. Parker (20) finds that the noise of motor boats is extremely faint under water and has almost no influence upon the movements of fishes engaged in feeding. Single explosive sounds like that of the report of a gun are likely to startle the fish and cause a temporary cessation of feeding.

Some tests made by Dr. A. G. Mayer, Dr. Goldfarb and the reviewer in Tortugas during the past summer on the distance at which sounds produced under water may be detected by a person with head and body immersed do not bear out Parker's contention that sounds heard under water are extremely faint. Dr. Mayer tapped two pieces of coral together under water while Dr. Goldfarb and the reviewer swam away. Up to a distance of approximately 100 yards the faint sound made in the above manner was distinctly heard by both observers with head and body immersed. At this distance Dr. Mayer tapped the two pieces of coral together in the air. They were clearly audible to the observers with heads in the air, but unfortunately we did not try the experiment of giving the sound in the air with the observers' heads immersed.

In Parker's work no mention is made of the possibility of vision being responsible even for the slight responses he obtained. In the opening sentences of the present paper he says that the fish can feel sounds through the *skin, lateral line organs* and through the ears proper. One of the strong criticisms urged against all of Parker's auditory work by certain German reviewers is that he does not exclude the possibility of response through vision (*i.e.*, visual response to actual wave motions produced by the sounding body). In the present paper, although he cites the work of Hunter (1782) on the responses of fishes to the noise produced by the discharge of fowling pieces, he does not consider the work of Bernoulli (Zur Frage des Hörvermögens der Fische, Arch. f. d. ges. Physiol., **134**, 633-644. See review of 1910 literature in this journal, Vol. 1, p. 436), who made similar experiments and arrived at similar conclusions.

Mammals. Shepherd (24) finds that the raccoon learns to respond to its own name and to other articulate sounds. Confusion words were not reacted to. Variations in the pitch of the voice and in the timbre of the voice did not cause failure to discriminate. This work seems to the reviewer merely indicative; in no sense can we say on the basis of such tests that Shepherd's conclusion is justified. The simplest and most fundamental precaution was neglected,—that of being out of sight of the animal during the experiments.

OLFACTION

Fish. Parker (19) tested the ability of Fundulus to discriminate between packets of cotton cloth containing dogfish meat and others not containing food. The packets without meat were occasionally approached and seized, but soon dropped. The killifish, in contrast with the catfish, used its eyes as well as its chemical senses in seeking food.

The olfactory nerves were then cut in several animals. Twenty-four hours after the operation the fish were fully active and took food. When tested with the food packets they nibbled at both in a way that made it impossible for the uninformed observer to distinguish one packet from the other. Since the operation of cutting the olfactory nerves was somewhat severe, Parker tested the effect of closing the anterior olfactory apertures by stitches of fine silk. Again discrimination between the packets was lost. It was restored when the stitches were removed. In cases where the stitches were taken, but where the threads were not drawn up and tied so as to close the aperture, discrimination was not lost. Parker concludes that *Fundulus heteroclitus* uses its olfactory apparatus as an organ with which to scent its food; "*i.e.*, its olfactory apparatus is a chemical distance-receptor of very considerable importance in its daily activities."[1]

Sheldon's (23) work on Selachians is in harmony with that of Parker. When a crab is placed in a pool containing dogfish a few minutes are necessary for diffusion to take place. The dogfish which first happens to swim within three or four feet of the crab turns suddenly as if startled, and swims about now eagerly alert and searches for the food. When within three or four inches of the crab it suddenly seizes it and makes off rapidly. Very little use of vision is noted. The food packet method was tried with convincing results. Packets containing stones were not touched, while those containing crabs were always seized. Packets saturated with the juice of a crab and weighted with a stone were likewise seized. When the nasal passages are closed with cotton wadding coated with vaseline, the fish do not respond to the food stimulus. The wadding produces some disturbance, but this factor was sufficiently controlled by testing fish with one aperture closed. From one to two days after the removal of the plugs fish so treated responded normally again.

The general conclusion is that Selachians find their food almost exclusively by a sense of smell which is comparable to that possessed by terrestrial vertebrates.

Birds. Strong (26) takes up the vexed problem of smell in birds from the standpoint of both morphology and behavior. His study of the anatomical features of the olfactory organs in birds leads him to agree with Edinger, *viz.*, that one ought to expect to find evidence of a sense of smell. There is a great variation in the size of the olfactory structures in the different species. Certain of the birds (Dromaeus and Fulmarus) have relatively larger olfactory apparatus than man. There is some evidence (according to his interpretation) in the case of the higher birds that the development of keen vision is being accompanied by a degeneration of the olfactory sense. On pp. 642 and 643 one finds a table of comparative measurements of the different parts of the olfactory structure, and on 659 and 661 excellent drawings of the olfactory organs and of their external relations to the brain as a whole.

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[1] Since neither Parker nor Sheldon mentions the previous work of Baglioni (Baglioni, S., Contributions expérimentales à la physiologie du sens olfactif et du sens tactile des animaux marins., *Archiv. Ital. de Biol.*, 1909, **52**, 225-230), I cannot help calling attention to the fact that this investigator, on the basis of tests upon the octopus and blind fish (Balistes), urges that these marine forms possess such a chemical sense, and on the question as to whether it should be called smell or taste comes out strongly in favor of calling it smell, since from the standpoint of reaction it is a distance-receptor.

On the behavior side, Strong presents some results obtained from experiments upon a maze which offered the possibility of association between a given odor and food. The birds never learned to find their food with perfect accuracy during a series of tests given twice daily and extending over a period of nine months. During this time, however, there was an increase in the number of correct responses to the oil of bergamot. Strong's field observations upon the turkey vulture in Florida were not productive of convincing results.

No control experiments upon birds with olfactory bulbs removed were made. It seems probable that Strong's tests could have been carried out much more quickly and easily if the two "choice" method instead of the four had been followed.

EXPERIMENTAL AND OBSERVATIONAL STUDY OF INSTINCTS

Fish. Sumner (27) in a rather loosely articulated paper gives the results of his highly interesting experiments upon the adjustment of the flatfish to its background. Pages 467-471 give a very clear summary. In brief, Sumner finds that flounders become very pale upon white backgrounds and very dark upon black ones. These adaptive changes usually lie within the black-white series. In mixed backgrounds composed of sand and gravel the fish respond by assuming definite patterns oftentimes in distinct harmony with those of the background. Backgrounds made up of areas of black and white call forth greater contrasts in the skin patterns than those of sand and gravel. Squares, crossbands. circles, etc., were never copied in any true sense. Adaptation is most complete upon backgrounds forming a part of the natural habitat of the animal, but it is not limited to such conditions. There is a wide range of adaptability.

The markings of the underlying surface were most effective in calling forth the changes. The markings on the vertical walls of the tank although effective were less important, while the overhead environment exerted little or no influence.

The fish seems to make no direct comparison between its body and the background. Adaptive changes occur when the fish is buried in the sand and when its body (eyes excepted) is covered with a cloth.

The changes in some way are conditioned upon optical processes, since at least one eye must fuction normally if the changes are to occur. If blinded when in the dark condition, the fish ordinarily remained dark; if blinded in the pale condition, they remained in that state for about a day and then reverted to a darker condition.

The author is conservative in his statements throughout and rests content largely with stating his findings. The photographs are excellent and on the whole convincing. The reviewer feels that the patterns shown in the photographs could have been somewhat better evaluated if Sumner had photographed along with the flatfish some fish rather constant in its color tone and pattern, and one which does not undergo such adaptive changes. In many cases, though, the author has photographed the flatfish in pairs in such a way as to leave no room for questioning the pronounced effect upon the pattern of the fish of changes in the backgrounds.

Werner (28) calls attention to the sleeping attitudes of several species of fishes (*Amiurus nebulosus*, *Misgurnus fossilis* and *Cobitus talnia*). Sleep is needed by all members of the groups particularly studied (*Silurus glanis* and *Cobitus barbatula*) but the bodily attitudes taken are quite different even in the nearly related races. In general there is a complete cessation of activity, a certain chosen bodily attitude, and almost complete stoppage of breathing. Very light contact is sufficient to reawaken the fish.

Romeis (21) takes up in detail the peculiar movements of the Maulbrüter (*Paratilapia multicolor*) in seeking during the breeding season a resting place under the leaves of certain water plants. When in the neighborhood of the thickly matted plant leaves, it interrupts for some seconds the lively play of its fins, swims slowly here and there, and when a suitable place is found it folds its fins, assumes a characteristic position (ganz schräg auf seine Breitseite gelagert) and remains in this attitude from one-half to two hours. Only the regular but slow movements of the gill cover show that life is present. Romeis doubts whether the above described form of sleep is very profound, since the near approach of any object, or noises, causes the fish to take flight immediately. The author sees in this activity a mechanism which is economical with respect to metabolism. It should be designated generally by "Ausruhstellung."

At night the behavior is different; the fish ordinarily sinks to the bottom of the container and rests on the ventral surface. Fin movements cease. It seems to be genuinely asleep, since light in moderate intensities and noises do not disturb it.

Amphibians. Mulder (18) finds that the speed of the rotation reflex is directly proportional to the amount of the stimulus. The relation is not logarithmic as is the case with sensations.

Birds. Breed (4) finds that the drinking instinct does not have to be supplemented by imitation, accident, intelligence, instruction, etc., in order to act. The pecking response as a whole is very imperfect at birth. For purposes of observation the response is subdivided into three separate reactions: striking, seizing and swallowing. The amount of daily improvement of the act as a whole and of its separate parts was measured up to the 25th day. Pecking improved in accuracy rapidly during the first two days. On the third day efficiency is placed at 29.29 on a scale of 50, 40.10 on the eleventh day and never higher than 42.57 for the rest of the time of observation. Social influence exerted little effect in improving accuracy.

On page 40 Breed raises the very interesting question of the relation of habit to instinct. Although in pecking there is improvement with practice, the question remains whether the increase in accuracy is dependent upon practice, or whether it is the natural functional correlate of structural maturation. The question might be answered experimentally, it would appear, by forced feeding of a group of chicks for the first week and then making a comparison of the accuracy of pecking with that of a group brought up in the normal way. Since the nervous system resists starvation well, it is doubtful if a few days of slight underfeeding would check the normal rate of the development of the sensory-motor arcs.

Craig (10) shows that stimulation of a female dove by the male without copulation was followed by oviposition. "The present paper goes to show that the stimulus to the whole process of egg development and laying is a psychic (neural) stimulus not dependent upon the introduction of sperm." (P. 305.)

Chappellier's (6) paper is a brief report upon a systematic study of the sex organs and sex life of hybrid birds. He discusses the apparent sex indifference in female hybrids obtained from crossing canaries and goldfinches. He mated one of these females with a canary. Nest-building began shortly after mating. The process of nest construction was desultory and lasted through the egg-laying period. The first egg was laid more than two months after mating. During 92 days she laid 16 eggs in a very irregular fashion and ate each of them in turn within one or two days after laying it. Although mated with an ardent male, she was entirely indifferent to him. This passivity in hybrid females is connected with a rudimentary condition of the ovaries, which are represented by three or four small formless masses.

Herrick (12) devotes nearly 100 pages to the nest structures and the activity of nest-building in birds. The series of papers is very comprehensive. He treats of the function of the nest, the classification of nests on the basis of behavior, analysis of increment nests, variations in nests of certain birds, nidification, intelligence in nest-building and the origin of the instincts of incubation and nidification. The discussion of nidification is based chiefly upon the writer's field observations of behavior in nest-building. The author is conservative in his discussion of the presence of intelligence in nest-building: "Instinct furnishes the springs of action in birds." These are exceedingly complex and in a high degree adaptable. This innate equipment takes the place in a large measure of mind or intelligence. He finds evidence of a wide group of associations between sense impressions and motor impulses. Habits of approaching and leaving the nest are quickly formed. Apparently one finds occasional adjustment of means to end where difficulties arise.

In regard to the origin of instincts of incubation and nidification, Herrick reaffirms his former position: the instinct to hide supplemented by that of guarding the eggs is responsible for the later instinct of incubation where guarding with concealment is effected in the same act. Why the instinct of incubation needs to have its "origin" accounted for any more than that of the hundred other instincts birds possess is not clear.

Craig (8, 9) gives the best description we have of the intimate family life of the mourning dove. He describes the various emotional states of the birds, such as those expressive of enmity, fear, alarm, and gives their various calls and cries in musical notation. The second part of the first paper (*The Expressional Life History*) treats of the breeding habits of the mourning dove. The breeding cycle of a pair of captive birds taken from Professor Whitman's aviary is described in detail.

His opportunity for making original observations upon the passenger pigeon was more limited. The species is now apparently extinct, and for this reason his rather brief notes upon the life history of Ectopistes are of especial interest. This species possessed characteristic voice, gesture and social life. It was comparatively awkward upon the ground and was therefore naturally less given to strutting and charging than other pigeons. Its voice was strident and lacking in musical notes. While the bird was quarrelsome, it was poorly equipped for fighting. Its courting was characterized by great roughness.

ORIENTATION

Mammals. V. Máday (16) discusses the problem of orientation in the horse. The origin of the impulse to home in this animal is supposed by the author to have been carried over from its wild ancestors. The *Heimweh* of the domestic horse has its origin in the *Wandertrieb* of its wild progenitors. This *Heimweh* is probably the most compelling impulse possessed by the horse.

Tests of homing were made by riding to distant points and then releasing the reins, thus allowing the horse to return by its own unaided efforts. The author tells us that he did not direct the horse except when gross errors were made! Surely the involuntary attitudes of the rider may have played a part both in the successes and the failures of the animal! When the horse reached the designated spot he was given the reins. The animal remained still for from one to two minutes, then started forward, then turned in the arc of a circle 10-15 m. in diameter and for a short distance retraced the outgoing course, finally choosing its own course so as to eliminate the detours made on the journey out. The journey was not made without error. Improvement by practice was noted. We are not given the details of the journeys—*i.e.*, distance traversed, times on successive trips, etc., nor the previous training of the horse. Several pages are devoted to discussion of theories, the senses of the horse, and the author's conclusions. In brief the latter are as follows.

Orientation is not a physiological reflex but a psychical process. All the senses play a rôle with the exception of taste. In the order of their importance in orientation the senses are listed as follows: smell, hearing, touch and sight. Direction, intensity and temperature of winds are important factors. It is curious that sight appears last in the list. He gives no experimental grounding for this particular hierarchy of the senses. From the author's note it appears that mistakes are often made when orientation is attempted through sight. In all of the higher animals orientation is a complex process involving both conscious and unconscious factors. The unconscious factor in orientation according to the author consists chiefly in a *Richtungsgefühl.* His discussion at this point is so naive and out of place in a journal with the standing of the *Zeitschrift für angewandte Psychologie* that the reviewer feels compelled to quote it in full: "Welcher Art is die bei der Orientierung geleistete unbewusste psychische Arbeit? Antwort: sie besteht hauptsächlich in dem sog. Richtungsgefühle.

Welche physiologischen Faktoren (Sinnesorgane) dienen dem Richtungsgefühle zur Unterlage und welcher Art sind die dabei entstehenden Empfindungen und Vorstellungen? Antwort: von verschiedenen Organen (hauptsächlich dem Ohrlabyrinth) kommen Lageempfindungen; auf diesen baut die Seele ihre Raumvorstellungen auf." (P. 85). In concrete cases, according to V. Máday, this *Richtungsgefühl* comes about through the unconscious registration of "detours" (Darwin). It is evident that the author was not really prepared, either from the standpoint of his familiarity with psychological theory or from his experimental equipment, to undertake such a problem. It is to be regretted that the journal in which the article appears will continue to give space to the reports of such unsatisfactory experimental work and to such long and rambling discussions as are to be found in this article and in the one by Franken, on the instincts and intelligence of the dog, reviewed last year in the Journal of Animal Behavior.

IMITATION

Mammals. Shepherd (25) gives some experiments upon imitation in raccoons. One of the animals had learned by the trial and error method to walk up an inclined plane of poultry netting 1.5 m. in length and 25 cm. wide to a box where food could be obtained. This animal was used as the "imitatee." Three other animals tested separately were allowed to watch the imitatee perform this feat. Since these animals repeatedly failed to profit by watching the trained animal perform the act, Shepherd concludes that "inferential" imitation does not form a part of the raccoon's mental equipment.

HABIT FORMATION

Amphibians. Schaeffer (22) finds that the three species of frogs studied (*R. clamata*, *R. sylvatica*, *R. virescens*) learned to avoid disagreeable objects, such as hairy caterpillars, in from four to seven trials. These habits persisted for at least ten days. Under the influence of punishment by the electric shock, *R. clamata* learned to avoid earthworms treated with chemicals in two trials.

Those habits which are intimately connected with obtaining food are acquired by these animals in a time which is directly comparable with the acquisition of similar habits in higher forms, *e.g.*, chick, tern, monkey, etc. The frog has been placed low in the scale of intelligence by other investigators. This is due to the fact that the problems presented it hitherto have called into play relatively few of its hereditary mechanisms. Two or three pages are devoted to the fruitless task of discussing the psychic processes in the frog.

Birds. Hunter (15) finds that the pigeon forms maze habits as readily as other birds, but not so readily as the rat and the squirrel. The maze habits are perfectly retained for four weeks. Visual and kinaesthetic cues are used in the learning process. Kinaesthesis does not play so important a rôle in the mental life of the pigeon as in that of the rodents. Although no especial tests were made upon audition, Hunter is in agreement with

Rouse that pigeons are very sensitive to sounds. From the tests with the rotated maze Hunter draws the conclusion that pigeons seem capable of changing their system of cues in response to the varying demands of a new situation. It is unfortunate that Hunter could not carry out these rotation experiments in an apparatus which permitted of the simultaneous rotation of the background.

Cole (7) continued the work of Yerkes and Dodson on the relation of the strength of the stimulus (punishment by electric shock) to rapidity of habit formation when the discrimination is (1) easy, (2) medium, (3) difficult. Chicks (barred Plymouth Rocks) were used as the subjects. The relative brightness of the stimulus plates in the three sets of tests is given as follows:

(1) Easy discrimination.................... 0: 8.9
(2) Medium " 1:13.7
(3) Difficult " 1: 5.1

Punishment was administered by grills connected with a calibrated inductorium. It was found that the number of trials necessary to form the habit of choosing the darker of the two screens when the difference in illumination is great decreases with the stimulus. This law holds, for discriminations of medium difficulty, only for the lower intensities of the stimulus. It holds likewise for difficult discriminations if the records of the chicks which succeeded in making it are alone considered. "If, however, we consider only the chicks which fail, the optimal stimulus recedes once more to a point nearer the threshold of stimulation than in the case of medium discrimination."

In the present state of the subject it is very doubtful if such results can be wholly relied upon. Quantitative experiments designed to isolate the effect of punishment are full of pitfalls. The effect is masked by practice effects, individual variation in animals, variations from day to day in the same animal, and by the almost insuperable difficulty of keeping an inductorium running for long periods of time without introducing serious errors due to the oxidation of the contacts through sparking, heating, etc. No condenser was connected across the terminals. Apart from the difficulties inherent in the method, Cole certainly worked with extreme care. The only criticism which can be urged in the conduct of the work was the *failure to carry out control tests without punishment in each of the three sets of experiments.*

Mammals. Hamilton's (11) paper voices a protest against the detailed and narrow experimental studies in behavior. His interest is not centered in the acquisition of definite habits, but rather in the reaction tendencies displayed by the animal when face to face with a situation for which it has no appropriate instinct, no opportunity for imitation, and no rational equipment. He states his problem as follows: "What, if any, are the qualitative differences of reaction tendency that account for the fact that some mammals learn slowly, and with many errors, to meet situations which their fellows of superior age or race learn to meet quickly and with few errors?" In order to gain insight into the different reaction tendencies of his subjects, he gives them a problem in which it is required to obtain exit from a room containing five doors. One of the doors is for entrance only. Each of the remaining four doors may give exit but only one gives exit in any given trial. The subject has to find which door gives exit. He must push against it: if he finds it locked he must try another. It will be seen at once that the type of problem is quite new.

The subjects chosen were human beings, monkeys, dogs, cats and horses, of different ages. From the varying behavior of these subjects he succeeds in isolating what he calls various types of reaction:

Type A: "The reaction must include a single, definite effort to open each of the three inferentially possible doors, and must not include an effort to open the inferentially impossible door." (The impossible door varied from trial to trial). This is *the rational inference type:* apparent only in the eight normal adult subjects.

Type B: This reaction involves trying all four doors but once each and in an irregular order: *The unmodified searching tendency.* It is exhibited by one defective and by all of the animals in the following order with respect to frequency of its display: monkeys, dogs, cats, horses.

Type C: Striking once, each of the four doors in succession, starting either left or right. (This can occur only when either door 1 or door 4 is locked). *The tendency to adopt stereotyped modes of searching*: especially exhibited by monkeys.

Type D: This involves the error of making more than one separate continuous effort to open a given door during the same trial, but always with an interruption of such repetitions of activity by an interval of effort to open one or more of the other doors. *The searching tendency modified by recrudescent motor impulses:* regularly increases as we descend the phyletic scale.

Type E: Includes several different modes of behavior which have a common objective characteristic, *viz.*, automatism. This is a relatively implastic and unadaptive mode of behavior. *The tendency towards perseveration of active impulses and inhibitions:* increases regularly when both phylogenetic and ontogenetic scales are descended.

Bogardus and Henke (3) studied in the rat the rôle played by head and nose contact in *learning* the maze. The vibrissae were cut off before experimentation began in order to facilitate the observation of the actual head and nose contacts made at the corners of the maze. It was found that the percentage of corners touched is high at first, and then gradually decreases as the maze is learned. There is a high correlation between the number of contacts and the number of errors made, and between the increase and decrease of time and the number of contacts. When the maze is learned, contact at corners is no longer necessary. Blind rats (previously trained on another maze) were then required to learn the maze used in the first set of experiments. The results obtained were similar to the above, and add weight to the contact theory of acquiring kinaesthetic and organic cues. This general conclusion is further strengthened by experiments in which the pathways of the maze were altered in various ways.

Hicks (14) gives a timely and exceedingly valuable discussion of the relative values of the different curves of learning. The author holds that the distance curve is a better representative of the progress towards automatic accuracy than the error curve. It, however, is impracticable from the standpoint of recording and manipulating the data. The prevalent practice of omitting all total and partial returns from the error record, and of making no attempt to evaluate varying degrees of error, gives a curve which is not only *worthless* but *false*. It is suggested that an error curve which includes returns, and which is constructed on the basis of regarding a small segment of the maze as the unit of error, constitutes a practicable substitute for the excellencies of the total distance curve. Time is the best single criterion.

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THORNDIKE'S "ANIMAL INTELLIGENCE"[1]

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Professor Thorndike's series of experimental papers, republished in this volume, constitute a conspicuous landmark in the development of comparative psychology. A review, thirteen years after the appearance of the first paper, can pretend to be little else than an appreciation in the light of subsequent progress. (1) The existence of the trial and error method of learning was demonstrated in fishes, chicks, cats, dogs, and monkeys. The random activities are an expression of the animal's innate and acquired nature, and any problem must be adapted to this nature. Motives of hunger, release from confinement, the social instinct, and preference for shade were utilized. Subsequent work has extended these tests and results practically throughout the zoological scale. (2) The analysis of imitation is excellent. The methods are those in use today. The results were negative for all animals studied. The conclusions as to inferential imitation have been substantiated by subsequent work. The author admits the possibility of other forms of imitation, and his statement that no influence upon conduct was evident in his tests must be interpreted in the light of the fact that his interest in inferential imitation would lead him to overlook slight influences of an instinctive and social nature. The tests were not always sufficiently controlled, but this defect was explicitly recognized, a statement which can not be made of some subsequent work. (3) The experiments on tuition have not stood the test of repetition and criticism. Again, the author was interested in the influence of ideas, and in this respect his conclusions seem valid. (4) Transference of training to similar problems was noted, but not much of the phenomenon is understood at present. The interpretation of the doctrine of recepts is probably the conventional one. (5) Difference of retentive ability was studied. Such tests have been duplicated, but practically nothing adequate is known of the retentive function as yet. (6) The discrimination experiments were poorly controlled. It is within this field that subsequent work has made its greatest progress. He used two methods, neither of which has been followed. In one a reaction to a given object is inculcated and then similar objects are interpolated in the series of tests. The second, so far as the distance senses are concerned, is much superior to those in use at present. A visual object "a" was placed against a defined background "b," and the animal was forced to discriminate "a" from "b." Recent unpublished work from this laboratory has demonstrated that when two spatially distinct objects are placed against an unknown background, as is commonly the case, one of the objects may be ignored. (7) The study of the instincts of chicks is excellent and most of the conclusions are yet valid. (8) The significance for intelligence of a development in motor capacity was strongly emphasized, a neglected topic in contemporaneous thought. (9) Animal learning was regarded as a process of association on the sensory-motor level. Animals may and probably do have ideas, but such ideas were not effectively present in their experimental behavior. So far as the reviewer is concerned, this conclusion is still valid, as no experiments yet published are convincing as to the presence of ideas.

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[1] Thorndike, Edward L. Animal Intelligence. New York, The Macmillan Company, 1911, pp. viii + 297. This is the third volume of the "Animal Behavior Series."

Such a summary in comparison with the present status of the science reveals the fact that these studies represent no inconsiderable achievement. Some new problems have since been attacked, *e.g.*, the relative values of the different senses in learning, etc.; the old problems and technique have been developed, but subsequent work has consisted to a large extent in a similar extended exploration of the animal scale. The author's success seems to be due primarily to his acute and thorough analysis of the human processes involved and to his keen appreciation of the logic of experimental procedure. While he robbed animals of their assumed ideational powers, yet he bridged the gap by his insistence upon the prevalence of the trial and error method of learning in the human mind.

Contemporaneous criticism was severe. The strictures as to the use of "utter hunger," an unfortunate expression, were unjustified so far as fact was concerned. Apology is made for the irritating dogmatic tone often displayed, and for the tendency to read severe advice to cognate disciplines. However, the contemporaneous attitude toward ideational interpretation of isolated cases of behavior *was* settled "once and for all" by these papers. The tendency to seek in experimentation an excuse to express views on the whole field of human and comparative psychology characterized other papers of this period, and in fact such a tendency is not without value in the development of any new field. Some interpretative positions were unfounded; the ideational significance of retentive permanence and of a sudden drop in the learning curve are doubtful propositions.

Two chapters are new. The first concerns the province of the science. It maintains the thesis that comparative psychology may deal with behavior for its own sake. Criticism is here futile, for we enter the realm of personal opinion. A better exposition of the validity of the objective method is difficult to find, but perhaps this statement is too much colored by the reviewer's bias. The chapter on the laws and hypotheses of behavior is one of the most important in the book, inasmuch as it expresses the author's mature opinion formed in the light of a decade of subsequent experimentation. The reviewer finds, however, more opportunity for dissent and criticism than in the earlier work. A few points may be noted.

(1) The law of causation holds sway; the behavior of any organism is wholly determined by the stimulus and the nature of the organism. This conception is emphasized because, it is asserted, two popular doctrines (in the sense of widely accepted, I presume) exist in defiance of it. The first is the doctrine that behavior is random. I deny that the term necessarily implies a meaning antagonistic to the above principle. A similar objection might also be made on just as good grounds to the phrase "accidental success" which is so liberally sprinkled throughout the book. That the term "random" can be employed without doing violence to the principle is evident from the fact that the author himself uses it twice (p. 255). If, as implied, any great number of writers actually do employ the term in the objectionable sense, I venture to suggest that a few names be specified. The second is the doctrine "that the need for a certain behavior helps to create it." This term can also be used without violating the principle. A "need" may represent the presence of a persistent irritating stimulus or the absence of a

customary stimulus, resulting in behavior of the trial and error sort which finally leads to accidental relief. Or, needs may be regarded as a constituent part of an animal's nature. On neither hypothesis does the concept of need violate the enunciated principle. Again, the specification of the names of a few of those who do use the term in the tabooed sense would satisfy one of the reviewer's needs. A suspicion is aroused that the author is here strenuously belaboring a straw man of his own manufacture.

(2) The learning process is explained under two main laws (effect and exercise) and two secondary laws (assimilation and partial activity). In the main the reviewer sympathizes with this formulation, but finds occasion for a few minor criticisms. (a) I doubt their comprehensiveness. I am not ready to admit that selection of acts according to their affective (pleasure-pain) consequences is the only principle of selection. The law of exercise is to some extent the application to animal acquisitions of the secondary laws of association. The law of exercise is inadequate in the field of animal learning, just as the secondary laws of association are inadequate to give a complete and comprehensive account of all the known conditions of human learning, *e.g.*, the phenomenon of the influence of the temporal distribution of trials or effort. My feeling is that the gradual selection and automatic growth of an act is too complex a matter and too little understood at present in many of its phases to be embraced adequately under the principles enunciated. Learning is conceived in too simple a manner. On page 280 is found the statement that the evolution of behavior is a simple matter; it reduces to differences of concrete particular connections and to differences in learning ability. This may be true, but I have found that the analysis of what constitutes degrees of learning ability is anything but a simple matter. (b) The law of effect states that in a series of random acts the selection of the proper or teleological act is made on the basis of the satisfying and dissatisfying consequences of those acts. What is satisfying or annoying can often be inferred on the basis of analogy with the human, *e.g.*, food or physical pain due to electric shock. In case this method fails to give a clue, we need some objective criteria. Satisfying conditions, we are told, are those which the animal tends to attain and preserve, and the annoying are those which are avoided and abandoned

(p. 245). In other words, we have to formulate a principle to explain the process of elimination and selection in a series of acts directed toward the solution of some problem, and we are told that the animal neglects, eliminates, avoids and rejects those acts which it avoids and abandons, and preserves and selects that act which it attains or preserves. This sounds suspiciously like the simplicity of alleged feminine logic which asserts that a thing is so because it is so. (c) That acts are selected according to their affective consequences may be admitted. Any explanation in terms of neural mechanics is a difficult problem, but one that should be attempted. A solution that fails to give the detailed mechanics of the process fails to simplify matters. I do not see that the principle of "utility to the neurones," which may be correct for all I know, gives this needed touch of simplification to the process.

(3) The doctrine that an idea of a response in and of itself is unable to produce that response already possesses a history of acrimonious discussion in the field of human psychology. The reviewer possesses no set of fixed ideas upon the subject, and the proposition hardly lends itself to an adequate critique in a short paragraph. It may not be amiss to state briefly what, I conceive, would be the answer of some of Thorndike's antagonists to this proposition. The author contends that the idea itself can not produce the act. The idea can do so only when a "bond of association" between the idea and the act has been established in past experience. The antagonists would assent to this latter proposition, and assert that that is exactly the reason why an idea of an act always does tend to eventuate into the act, that ideas of acts always do possess these "bonds" with the acts because an idea of an act can arise only from past experiences with that act. They would assert that it is this bond with a particular act that makes the idea an idea *of that particular act* rather than an idea *of something else.* In other words, they would assert that "this idea of an act without a bond" is a mythical entity of Thorndike's creation which no more exists than does the Jack of Spades. Such a mythical idea might for the sake of the argument be called an idea, but it could not properly be termed an idea *of* anything. While all ideas of acts do have bonds with those acts because of past experience, yet there is a sense in which an idea of an act can

produce the act without a *direct* bond. This is the case of the ideational construction or alteration of a new act out of old acts. Suppose we have ideas "a" and "b" of acts "A" and "B" with the given bonds of connection. The situation necessitates the creation of a new act "C" by coördinating A and B in some way. It might be asserted that this coördination may be perfected on the level of their ideational representatives "a" and "b," and, when perfected, the act C would result although no *direct* bond exists between the new idea and the new act. Otherwise, ideas would possess but little value in volitional activities. In fact, as the reviewer read the author's discussion, he began to wonder whether ideas could possess any utility whatsoever in Thorndike's system. The suspicion was engendered that ideas are mere by-products of sensory-motor connections, and that the author could give a smooth account of all the processes involved and of the development of intelligent behavior in both man and beast without the least reference to ideas.

HOLMES'S "THE EVOLUTION OF ANIMAL INTELLIGENCE"[1]

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Professor S. J. Holmes, a biologist whose work is highly esteemed in France, has recently published a very interesting work on comparative psychology, entitled "The Evolution of Animal Intelligence." In order to demonstrate this evolution, Holmes studies a certain number of types, chosen in such a way as to bring into evidence the activities from which intelligence is derived; certain groups of the animal kingdom are left entirely out of account, because they are hardly adapted to furnish useful data towards the solution of the problem which the author has set himself.

From the outset, Holmes clearly shows what his guiding ideas are. The ingenious theory wherein he makes pleasure and pain play an important rôle is already familiar to us: these are the powerful agents of inhibition and reinforcement, and consequently of adaptation and evolution; the development of pleasure-pain reactions constitutes the most important stage in the evolution of "behavior," for the development of intelligence itself is, according to the theory, based on these reactions.

But to speak of pleasure-pain means to raise at once the question of *consciousness* and *sensations* in the lower animals, questions which have already given rise to so much controversy. We have certainly no means of knowing states of consciousness outside of ourselves. Holmes does not conceal this fact from himself, but it does not prevent him from continuing the discussion. The analogies between man and the higher animals are so evident as to make it certain that the latter are conscious of some of their acts. In the case of lower forms, we are reduced to probabilities. Certain writers, among whom the author is mistaken in citing me (for to me there is no objective mark of consciousness), have regarded ability to learn, or associative memory, as a criterion of consciousness. Romanes and Morgan hesitate to conclude that there is no consciousness where there is no associative memory. According to Holmes, it is probable that all animals which learn are conscious, but such is not necessarily the case; moreover, we must not deny consciousness to animals that cannot learn. But are there such animals? I think not: there are, as Margaret F. Washburn has well shown in her fine book, "The Animal Mind," various ways of learning. Among these ways, one of the most perfect is "associative memory." Now my researches on actinians and the very recent ones of Metalnikow on infusoria show that associative memory functions even in the lower organisms, and is consequently a general property of living matter. We should thus be led to admit that a sort of consciousness accompanies all organic processes, a conclusion calculated to satisfy those who allow no breach of continuity in the sequence of vital manifestations.

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[1] Holmes, S. J. The Evolution of Animal Intelligence. New York, Henry Holt and Company, 1911, pp. v+296.

Holmes devotes two chapters (II and III) to Reflexes and Tropisms. According to him, the writings of Jacques Loeb, and in particular Loeb's book on heliotropism in animals, by the interesting facts which they have made known, the ingenious experiments which they have suggested, and the general theory to which they have led, have had a considerable and stimulating influence. We must not rest contented with describing the facts of behavior; we must seek to explain them. Holmes then examines the criticism of Jennings to the effect that in certain alleged tropisms there is no definite orientation. He next passes to an examination of the variations in phototropism, and speaks in particular (p. 50) of some unpublished experiments by Michener, according to which chemicals cannot make a reaction negative which was primitively positive. But Anna Drzewina has recently succeeded in producing this result by using a substance, potassium cyanide, which inhibits oxidations; and acids act in the same way when their action is prolonged.

The next chapter (IV) treats of the "Behavior of the Protozoa." The reader has already had an opportunity, in the *Biological Bulletin* for 1907, to become acquainted with the author's very interesting considerations on the rhythmic activity of certain infusoria (Loxophyllum). Holmes thus treats this question with especial competence. He recalls at the outset the memory of our regretted Binet, and discusses at length the possibility of choice in the infusoria. According to Mast, who is here in essentials repeating an opinion of Maupas's (expressed in my "Naissance de l'Intelligence"), the choice is only apparent, and is due, for example, to the fact that the prehensile organ adheres to certain organisms and not to others. But Metalnikow has just shown that if the infusoria do not in general choose their food, they can at least *learn* to choose it.

With Chapter V the discussion on instinct begins. Holmes recalls the fact that I have regarded "instinct" as a word adapted merely to hand down to us certain old notions which flourished before the introduction of experimental analysis in comparative psychology. He himself is a partisan of the conservation of this word. The animal, he justly observes, is not merely a machine which responds to the various influences of the environment: it possesses an innate stock of impulses, which lead it to act in a definitely determined manner, independently of stimulations from the surroundings. From the beginning of its life, we must take account of *internal impulses* (the *Triebe* of the German writers). Instinct represents this innate stock of impulses: to keep the notion of instinct means to show that we do not fail to recognize *the importance of internal states, of internal factors.* In other respects, the activities determined by internal factors do not differ fundamentally from the activities called reflex. The stimuli are internal instead of external.

Holmes speaks of the "marvellous adaptation" of instinct; yet he recognizes that in many cases it is far from being perfect, and he shows us instincts which are in process of perfecting and evolving (Chapter VI).

Two very interesting chapters are VII and VIII, which treat of "Non-intelligent Modifications of Behavior," and of "The Pleasure-pain Theory." According to Holmes, even among the lower organisms there is a sort of auto-regulation of actions, which leads to adapted responses: these responses have contributed to the development of intelligence, which in its turn has had its full expansion when the adaptations have become much more varied through the perfecting of the formation of associations. Many facts bearing on this subject are set forth in a series of paragraphs entitled: "Action of Combined Stimuli," "Diminution of Reaction to Repeated Stimulation," "Different Kinds of Response to a Given Stimulus," "Influence of Internal Factors on Behavior," "Habits," "Rhythms in Behavior." It was with pleasure that I saw mentioned and discussed some of my own researches, in particular those on the temporal variations of sensibility in Cerianthus, and on tidal rhythms. The following appears to me to be a very general law: in proportion as external excitations are repeated or prolonged, sensibility at first increases and later diminishes, the first phase being shorter as the organism is less rich in certain active substances. One gets a glimpse here of the interest which attaches to physico-chemical explanations, which Holmes does not reject, but of which he takes little account, no doubt because they are still a little premature. On the subject of rhythms, I should like to remark that there are strong rhythms and weak rhythms; the latter may be easily masked by the causes actually in operation, and great precautions, which certain authors (Morse in particular) have failed to take, and which I have indicated in my articles (sensibilisator, maintenance of constant illumination and hygrometric state, etc.) are necessary to demonstrate their existence. I shall not revert to the pleasure-pain theory, of which I have spoken above and which is well known to the readers of this review, since it was set forth at length by its author in the *Journal of Comparative Neurology and Psychology* for 1910. With pleasure-pain reactions intelligence came into being. Holmes justly rejects the old opposition of instinct to intelligence: there are already present along with instinct the rudiments of intelligence when the animal profits by experience through the formation of associations; from simple associative memory it is possible to pass through all the intermediate stages to the complicated forms of reasoning. These are the views which I also have maintained in my books, "La Naissance de l'Intelligence" and "La Nouvelle Psychologie Animale."

In the following chapters (IX to XIII), Holmes seeks to show us the evolution of intelligence; with Bethe, Spaulding, and A. Drzewina, he presents to us the primitive forms of intelligence in crustaceans and molluscs, and discusses the intelligence of insects, lower vertebrates, mammals, and, lastly, the monkeys. In treating of the mammals, he insists on the necessity of experiments: he discusses in particular those of Thorndike, his imitators and pupils. But experiments should be conducted with discrimination, and caution must be observed in drawing conclusions from them. We must not forget, as certain psychological trainers of animals have done, that "the lion of the desert is quite a different creature from the lion of the circus." Holmes discusses the formation of ideas, reasoning, and imitation in the higher animals, and reports the exploits of the monkey Peter, whom an illustration shows in the act of dining. He brings the book to a close without formulating his conclusions, but they are apparent as one reads it.

Some surprise may be felt that Holmes fails to mention the work of Pawlow's school, which deals with conditioned reflexes where the method of psychic salivation is used. These experiments, which furnished the stimulus for the recent researches of Metalnikow on the infusoria, have the great advantage of helping us to penetrate into the determinism of animal behavior, and making us understand how far such behavior is from being adaptive. In my opinion, Holmes lays too much stress on adaptation, the finality of acts: in most cases this is but a delusion and a snare. Intelligence itself is for us only a very imperfect instrument.

However, Holmes's book is very valuable; its facts are well selected, its ideas interesting and original. Every biologist and philosopher should read it.

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